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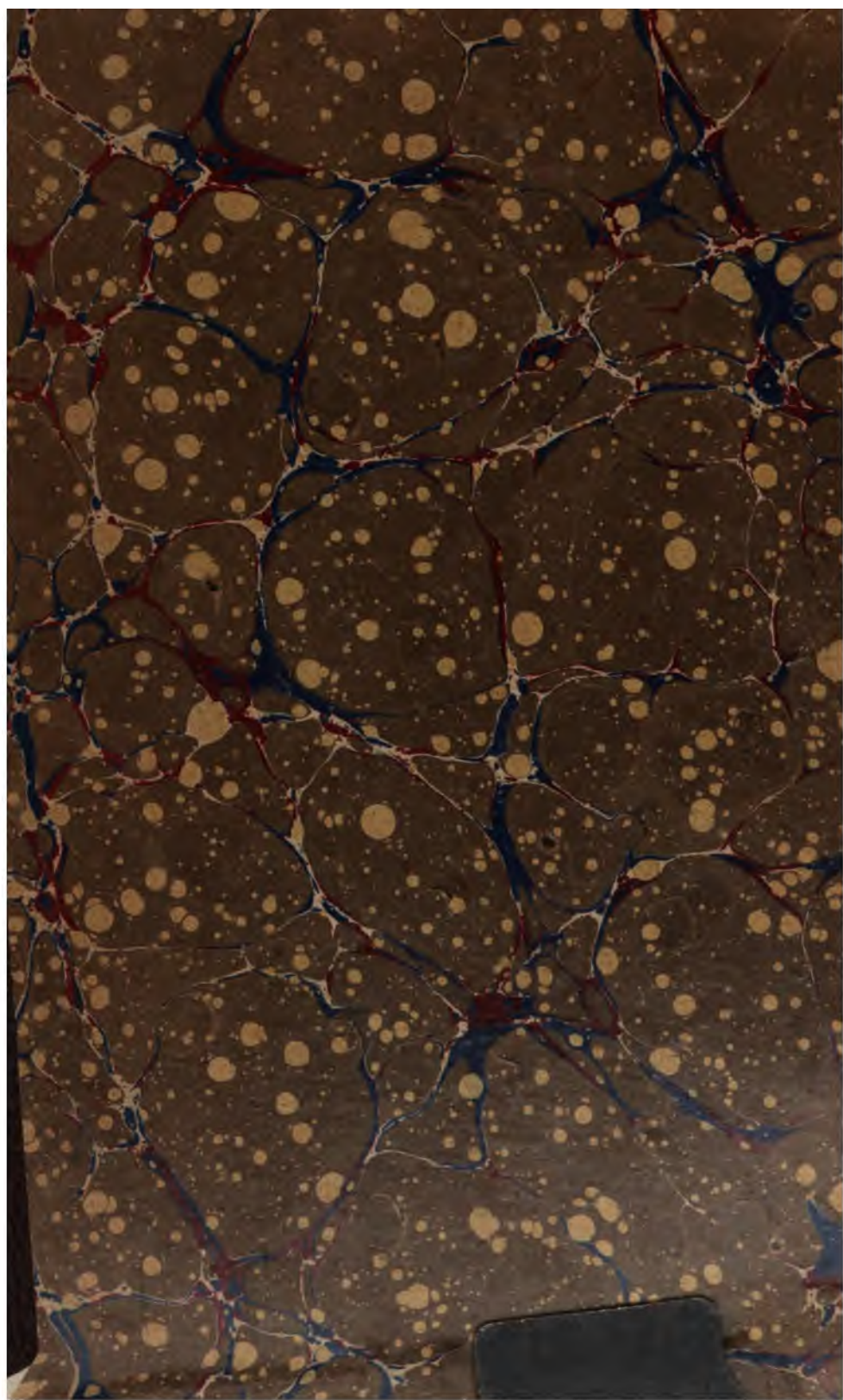
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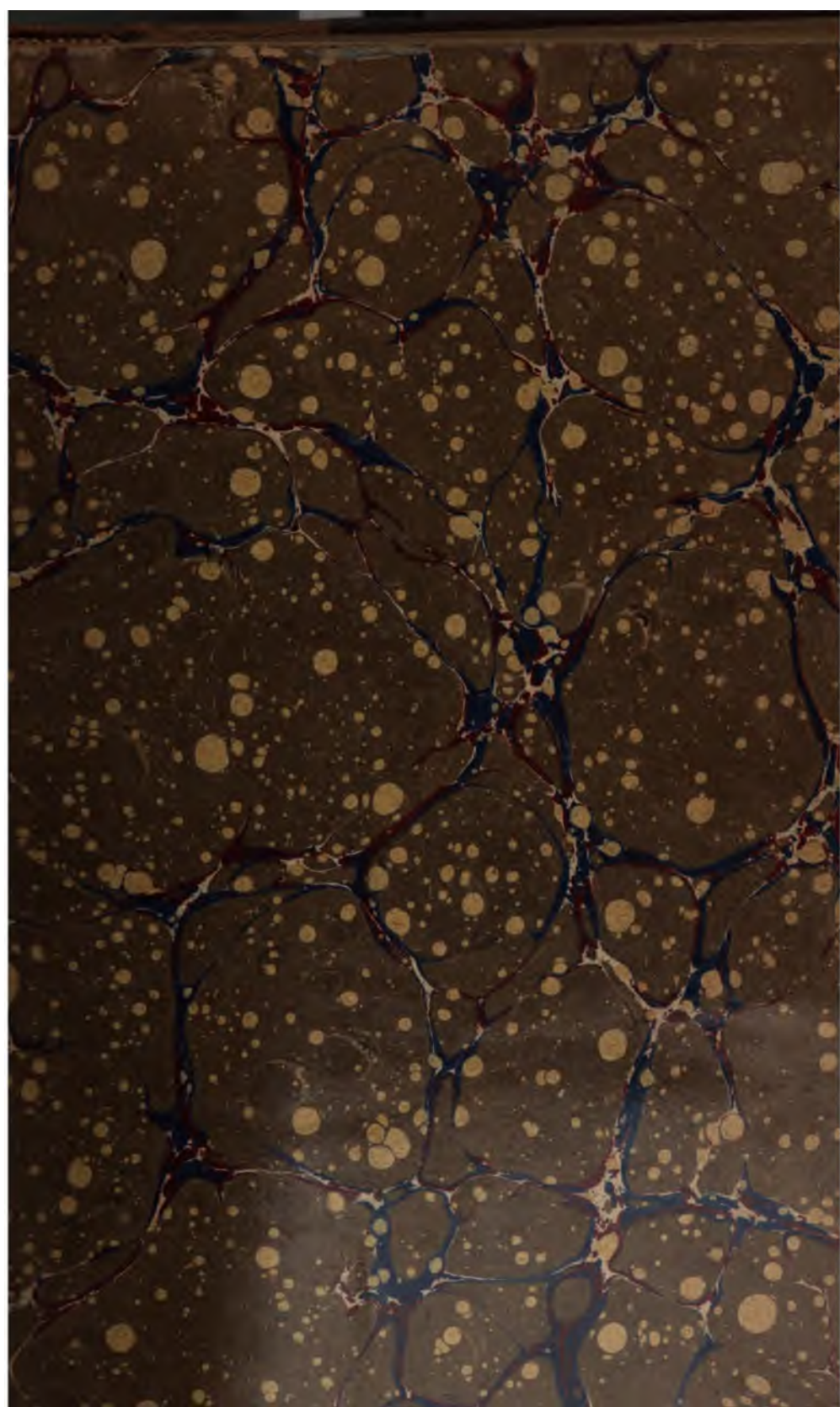
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MEMOIR
OF
WILLIAM BARTON ROGERS.
1804-1882.

BY
FRANCIS A. WALKER.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1887.

Copyright, 1887,
by Francis A. Walker,
Author of "The Life of William Barton Rogers,"
and "The Life of William Barton Rogers,"
and "The Life of William Barton Rogers."

YHABBU!
ROBIL GROMATZ OBA.B!
YHABBU!

BIOGRAPHICAL MEMOIR OF WM. BARTON ROGERS.

MR. PRESIDENT AND GENTLEMEN OF THE ACADEMY:

William Barton Rogers, the third President of the National Academy of Sciences, was born December 7, 1804.

His father, Patrick Kerr Rogers, a native of Newton Stewart, in the north of Ireland, had become, while yet a student at Trinity College, Dublin, suspected by the Government, by reason of his patriotic affiliations, and, in view of the political persecution to which he was thus exposed, emigrated to America. Here he took up his residence in Philadelphia, receiving the degree of doctor of medicine on the completion of his studies in the University of Pennsylvania. Dr. Rogers began the practice of medicine in Philadelphia. Here he married Hannah Blythe, a lady of Scottish birth, and here were born his eldest son, James, and his second son, Wm. Barton, the subject of this memoir. Two other sons, Henry D. and Robert E., were afterwards born to Mr. and Mrs. Rogers. These four brothers were destined to form a family group scarcely to be excelled for native powers and acquirements, in the history of science, in this or any age or country. The death of his father called Patrick Kerr Rogers back to Ireland. Finding, upon his return, that his long absence had impaired his professional practice, Dr. Rogers determined to remove to Baltimore, but soon afterwards accepted the professorship of chemistry and physics in William and Mary College, Williamsburg, Va. Here he resided during the remainder of his life, and here his four sons were educated. Upon his death, in 1828, Wm. B. Rogers succeeded to his professorship.

Wm. Rogers had already had experience in teaching science, having, during the preceding year, carried on a course of lectures in the Maryland Institute, where he first displayed, upon an adequate field, that power of clear exposition and felicitous illustration which he possessed in a degree, perhaps, never excelled.

As professor of physics and chemistry, we find him early publishing papers on the physical side of his department, one upon Dew and one, prepared in conjunction with his brother Henry, upon

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the Voltaic Battery; but his attention was at first bestowed, in greater measure, upon chemistry, and this, too, with reference to industrial applications. For the Farmers' Register he wrote a series of articles on the Green Sand and Calcareous Marl of Eastern Virginia and their value as fertilizing agents, which aroused interest very widely, and contributed largely to awaken that desire for the scientific and economic exploration of the State which led to the establishment of the geological survey of Virginia.

This special and strong interest in the economic bearings of science characterized the entire career of Prof. Rogers early and late. He, of all men the least prosaic, gifted with a fervent imagination such as is rarely coupled with the disposition and capacity for patient and protracted research, valued science not more for the sake of the truth than for the sake of the virtue which is to be found in it for the amelioration of the human condition. Always had those investigations a doubled attraction for his mind which, promised to place new resources at the disposal of mankind.

In the year 1835 Prof. Rogers was called from William and Mary College to the University of Virginia, to fill the chair of natural philosophy and geology in that already eminent institution. During the same year he was appointed geologist of Virginia, a survey of the State having been provided for by the Legislature, chiefly in consequence of papers printed and addresses delivered by him.

While vigorously prosecuting the work of the geological survey during the several years succeeding Prof. Rogers carried on, without serious interruption, the duties of his professorship. The traditions concerning his lectures, which still linger around the halls of the University of Virginia, tell of a force of statement, a felicity of illustration, a power of eloquence marvelous to hear. Says one of his former pupils, Wm. LeRoy Brown, President of the Agricultural and Mechanical College of Alabama:

"I remember well the very great interest in and enthusiasm for science he excited among the students by his brilliant lectures. Often, especially when it was announced that he would begin his lectures on astronomy, have I seen his lecture-hall crowded with students from other departments, including those of law and medicine; indeed, so crowded with young men, eager to hear the eloquent presentation of the subject by the professor whom they so greatly

admired, that not even standing room could be found in the hall. All the aisles would be filled, and even the windows crowded from the outside with eager listeners. In one instance I remember the crowd had assembled long before the hour named for the lecture, and so filled the hall that the professor could only gain admittance through a side entrance leading from the rear of the hall through the apparatus room.

"His manner of presenting the commonest subject in science—clothing his thoughts, as he always did, with a marvelous fluency and clearness of expression and beauty of diction unsurpassed—caused the warmest admiration, and often aroused the excitable nature of Southern youth to the exhibition of enthusiastic demonstrations of approbation. Throughout Virginia—and, indeed, the entire South—his former students are scattered, who even now regard it as one of the highest privileges of their lives to have attended his lectures."

Traditions of the power of the orator, the legal advocate, the parliamentary leader, the philosophic reasoner, are rarely at fault; and were we to depend on the testimony of his former students at the University of Virginia alone we need not entertain a doubt that Prof. Rogers was gifted, almost beyond the privilege of man, in the exposition of scientific truth. But he was yet to have a far wider audience, and everywhere, whether before the British or the American Association, or in still another institution of his own founding, or in learned societies not a few, or in this Academy of Sciences, over which he presided, he was to win continually new and higher triumphs.

To this contributed, not alone the *perfervidum ingenium* of his race, not alone an imagination which ever clothed truth with beauty, and made the dullest fact radiant with a significance illimitable and imperishable, but also every personal gift which can enhance the power of the orator. Tall in stature; with a figure of the type known to us through the pictures of Henry Clay; with a face that, destitute of all assumption or arrogance, was singularly commanding; with a voice whose compass and quality were capable of producing at once the largest and the finest effects of speech, Wm. Barton Rogers was, in the height of his powers, without a peer among the scientific men of his age in addressing an intelligent and cultivated audience.

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But while Prof. Rogers was thus delighting and entrancing the students of the University of Virginia with his lectures on astronomy and physics, he was, in those early days of science, carrying forward, in co-operation with his brother Henry, who simultaneously held the office of State geologist of Pennsylvania, one of the most important enterprises in the history of geology. To these two brothers, knit closely by intellectual as well as by moral sympathies, the world owes the unfolding of the great Appalachian chain. Each laboring in his own field, yet prompt to communicate to the quickened apprehension of the other the ripe results of his investigations, the earliest suggestions of his daring mind, William and Henry Rogers did a work for American geology, between 1835 and 1842, the importance of which has not been obscured, the essential accuracy of which has not been impeached, by the labors of their professional brethren, in these days of the fruition of science.

The main features of Prof. Wm. Rogers' work as the State geologist of Virginia, whether by himself alone or in conjunction with one or another of his gifted brothers, has been thus summarized by a competent authority:

"In connection with his brother Robert, Prof. Wm. B. Rogers was the first to investigate the solvent action of water—especially when charged with carbonic acid—on various minerals and rocks; and by showing the extent of this action in nature and its influence in the formation of mineral deposits of various kinds, he was one of the first to observe and interpret the important class of facts which are the basis of chemical geology.

"Another important result of Prof. Rogers' geological work was to show that the condition of any coal-bed stands in a close genetic relation to the amount of disturbance to which the enclosing strata have been submitted, the coal becoming harder and containing less volatile matter as the evidence of disturbance increases. This generalization, which seems to us now almost self-evident—understanding, as we do, more of the history of the formation of coal—was with Prof. Rogers an induction from a great mass of observed facts.

"By far, however, the most memorable contribution of Prof. Rogers to geology was that made in connection with Henry D. Rogers in a paper entitled 'The Laws of Structure of the more Disturbed Zones of the Earth's Crust,' presented by the two brothers at the meeting of the Association of American Geologists and Natu-

ralists, held at Boston in 1842. This paper was the first presentation of what may be called in brief the Wave Theory of Mountain Chains. This theory was deduced by the brothers Rogers from an extended study of the Appalachian chain in Pennsylvania and Virginia, and was supported by numerous geological sections and by a great mass of facts. The hypothesis which they offered as an explanation of the origin of the great mountain waves may not be generally received, but the general fact that the structure of mountain chains is alike in all the essential features which the brothers Rogers first pointed out has been confirmed by the observations of Murchison in the Ural, of Darwin in the Andes, and of the Swiss geologists in the Alps. The wave theory of mountain chains was the first important contribution to dynamical and structural geology which had been brought forward in this country. It excited at the time great interest, as well from the novelty of the views as from the eloquence with which they were set forth, and to-day it is still regarded as one of the most important advances in orographic geology.

"A marked feature of mountain regions is that rupturing of the strata called faults, and another of the striking geological generalizations of the brothers Rogers is what may be called the Law of the Distribution of Faults. They showed that faults do not occur on gentle waves, but in the most compressed flexures of the mountain chains, which in the act of moving have snapped or given way at the summit where the bend is sharpest, the less inclined side being shoved up on the plane of the fault, this plane being generally parallel to, if it does not coincide with, the axis plane; and, further, that 'the direction of these faults generally follows the run of the line of elevation of the mountains, the length and vertical displacement depending on the strength of the disturbing force.'"

The last of the general geological results to which we referred above was published under the name of Wm. B. Rogers only. It was based on the observed positions of more than fifty thermal springs in the Appalachian belt, occurring in an area of about fifteen thousand square miles, which were shown to issue from anticlinal axes and faults, or from points very near such lines.

Prof. Rogers' active labors in connection with the geological survey of Virginia ceased in 1842. The successive annual reports which he rendered to the Legislature of the State are models of

clear statement and luminous exposition. After becoming, through a whole human generation, increasingly rare and difficult to obtain, though never antiquated, but, on the contrary, continually gaining interest and value, as the work of his successors in the field of American geology brought into bolder relief the marvelously just and felicitous conceptions of those pioneer explorations, these reports have recently been reprinted under the careful editorship of Mrs. Rogers, aided by Major Hotchkiss, the well-known geologist of the Virginias.

Throughout his labors in the field Prof. Rogers continued his service in the University of Virginia, each successive class being to him a new band of pupils and friends, to whom he delighted to expound the truths of science.

After the completion of the geological survey and the appearance of its last official report, Prof. Rogers continued through several years to publish papers based on the results of the survey or of fresh investigations suggested thereby.

Three of these—"On the Age of the Coal Rocks of Eastern Virginia," "On the Connection of Thermal Springs in Virginia with Anticlinal Axes and Faults," and "Observations of Subterranean Temperature in the Coal Mines of Eastern Virginia"—appeared in the reports of the American Geologists' and Naturalists' Association, in the foundation of which, in 1840, he took a deep interest and over which, in 1845, he presided.

In 1843 he published in the proceedings of the American Philosophical Society a paper "On the Phenomena of the great Earthquakes which occurred during the past winter—one in this country and the other in the West Indies—and on a general theory of Earthquake Motion." In 1844 he published in Silliman's Journal "A System of Classification and Nomenclature of the Paleozoic Rocks of the United States," with an account of their distribution, more particularly in the Appalachian Mountain chain. The last of his papers showing a distinct relation to his geological work in Virginia was published in the proceedings of the American Association for the Advancement of Science, in 1848, "On Acid and Alkaline Springs." Prof. Rogers also published a series of papers on chemistry, the results of investigations conducted by him in close conjunction with Robert E. Rogers, afterwards professor in the University of Pennsylvania and later still in the Jefferson Medical College of Philadelphia. The same remarkable power of collaboration which had been exhibited in the geological enterprises conducted jointly by

WILLIAM BARTON ROGERS.

William and Henry Rogers here again appeared in the common efforts of William Rogers and his youngest brother.

Moreover, during his connection with the University of Virginia, Prof. Rogers published a short treatise on the Strength of Materials and a volume on the elements of Mechanical Philosophy, of which a competent authority has recently said: "It is far superior to any other elementary work that I have ever seen, in the lucid explanations which are given, and in the distinctness with which the leading principles of the science are enunciated, reminding one of the best style of Dr. Thomas Young."

In 1849 Prof. Rogers married Miss Emma Savage, of Boston, daughter of the eminent genealogist, Hon. James Savage, LL.D. The history of thought and research bears testimony on many a page to the aid which high-minded and devoted women have rendered to the cause of science, in soothing and solacing, in tending and comforting, in encouraging and sustaining the overwrought and much-perplexed students of natural laws; but that history contains no record of a nobler or happier companionship than that which began with the connection thus formed.

In the year of his marriage Prof. Rogers visited England and took part in the proceedings of the British Association for the Advancement of Science, in Birmingham.

In 1853 Prof. Rogers resigned his professorship in the University of Virginia and took up his residence in Boston, where he early associated himself with the American Academy of Arts and Sciences and the Boston Society of Natural History, taking an active part in the proceedings of both these learned societies, in the latter in close communication with Agassiz, Wyman, and Jackson. To this period belong the following papers:

Proofs of the protozoic age of some of the altered rocks of Eastern Massachusetts from fossils recently discovered. (*Amer. Acad. Proc.*, 3, 1852-'7, pp. 315-'18.)

On the relations of the new red sandstone of the Connecticut Valley and the coal-bearing rocks of Eastern Virginia and North Carolina. (*Silliman's Journ.*, 19, 1855, pp. 123-'5.)

On the Paradoxides found in the altered rocks of Eastern Massachusetts. (*Edinb. New Phil. Journ.*, 4, 1856, pp. 301-'4.)

Discovery of paleozoic fossils in Eastern Massachusetts. (*Silliman's Journ.*, 22, 1856, pp. 296-'8.)

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On the origin and accumulation of the protocarbonate of iron in coal measures. (Silliman's Journ., 21, 1856, pp. 339-'43.)

On the causes which gave rise to the generally elongated form and parallel arrangement of the pebbles in the Newport conglomerate. (Silliman's Journ., 31, 1861, pp. 440-'2.)

His work during this period, however, was mainly in physics.

He studied the variations of ozone (or what was then regarded as ozone) in the atmosphere at the time when that subject was attracting wide attention.

He was greatly interested in the improvements of the Ruhmkorff coil made by Mr. E. S. Ritchie, and in this connection published a paper on the "Actinism of the Electrical Discharge in Vacuum Tubes." An investigation of some of the phenomena of sight led to a series of papers on binocular vision, which were subsequently reprinted in a separate volume. A paper discussing the phenomena of smoke rings and rotating rings in liquids appeared in the American Journal of Science for 1858, with the description of a very simple but effective apparatus by which the phenomena might readily be reproduced. In this paper Prof. Rogers anticipated some of the later results of Helmholtz and Sir William Thomson. In the same year an ingenious illustration of the properties of sonorous flames was exhibited to the Warren Club of Boston, in which Prof. Rogers anticipated Count Schafgotsch in the invention of a beautiful optical proof of the discontinuity of the singing hydrogen flame. The subject was subsequently treated by Prof. Rogers in Silliman's Journal.

In 1861 Prof. Rogers was appointed by Governor Andrew inspector of gas and gas meters for the State of Massachusetts, and at once applied his trained and ingenious mind to the creation of a system of inspection which should give the community the benefit of the latest results of science in this direction. These researches led to a visit to Europe in 1864, during which he presented to the British Association at Bath a paper entitled "An Account of Apparatus and Processes for Chemical and Photometrical Testing of Illuminating Gas."

In 1859 Prof. Rogers, gathering around him a number of the first citizens of Boston, began the public discussion of a scheme for technical education, to be associated, on the one side, with research and original investigation upon the largest scale, and, on the other, with agencies for the popular diffusion of useful knowledge. So entirely

WILLIAM BARTON ROGERS.

unfamiliar to the public mind of the day was the idea of technological instruction, beyond the simplest requirements of civil engineering, that the Legislature of Massachusetts could not be brought to see the full merits of Prof. Rogers' most comprehensive and, as all now view it, thoroughly practical plan, but enough was done by the Legislature during the few years following to secure the chartering, in 1862, and the actual inauguration, in 1865, of the Massachusetts Institute of Technology, of which Prof. Rogers became the first president, devoting to it all the energy and enthusiasm of his impulsive nature and all the varied wealth of his accomplishments and acquirements. For the rest of his life this was his chosen work.

The present is not the time, nor am I the person, to speak of the development of Prof. Rogers' plans.

One thing should, however, be mentioned. In the Institute of Technology the laboratory methods of instruction were first applied, in large classes, to the teaching of physics. Prior to this, physics had been taught in the lecture-room alone. The only laboratory was that in which the Professor and his assistants conducted their researches or made preparations for illustrated lectures. At the most, and that very rarely, there was room and opportunity for a few advanced students to participate in the investigations of the teacher. In his new school Prof. Rogers at the outset proposed the introduction of laboratory methods in physics equally as in chemistry. In pursuance of this scheme Prof. Edward C. Pickering, then in charge of the department of physics, developed a system by which the largest classes could be trained to make physical observations and measurements, and gradually brought to the capability of conducting investigations with due regard to the conditions of a conclusive experiment.

So completely were his activities and sympathies enlisted by his new work that, in 1868, Prof. Rogers came under a disability, which, for the ten years* following, rendered intellectual exertions highly dangerous to life.

* In spite of his disability, however, Prof. Rogers, in May, 1875, presented to the Natural History Society two elaborate papers, one on the Newport Conglomerate, the other on the Gravel and Cobblestone Deposits of Virginia and the Middle States.

Between 1867 and 1876 Prof. Rogers, at the request of General Hum-

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Mercifully recovered therefrom, under a personal care and attendance such as is seldom vouchsafed to invalids, he, in 1878, resumed the presidency of the Institute, although under stringent limitations as to labor and excitement. On the death of the illustrious Henry, he was elected, in 1879, President of the National Academy of Sciences, of which he had been one of the charter members; and surely, brethren of the academy, few have been the men who would take a keener delight in presiding over the discussions of a learned society, dealing with the whole range of exact knowledge, or who were more amply qualified and endowed for leading and inspiring the deliberations of such a body.

The wide extent of his own studies and researches in mechanics, physics, chemistry, and geology; his truly philosophical spirit, his unfailing courtesy and urbanity, his warm sympathies, his scientific enthusiasm, his commanding and stately presence, his rare gifts of expression, all combined to make him the ideal presiding officer. His introductions were most felicitous; his comments highly suggestive and inspiring; his summing-up was always a masterpiece of discriminating and judicious reasoning, while, over all, his rich tropical eloquence threw a spell as of poesy and romance, for to him the truth was always beautiful, and the most solid and substantial structure of scientific principle stood in his view against a sunset sky, radiant with a light which no painter's pencil ever had the art to fix on canvas.

In 1881 his increasing infirmities led him to resign to another the presidency of the Institute of Technology, in which, however, in testimony of his unabated interest and devotion, he remained professor emeritus of physics and geology. When thus he was compelled to withdraw from his more active duties, thousands of well-wishers, alike in the fields of his earlier and of his later labors, hoped for a long twilight of quiet happiness and mellowed glory, amid all that could soothe and cheer and charm the decline of life; but the end of this career of honor and usefulness was at hand, and that end was to be sublimely fitting and appropriate to that career.

phreys, Chief of Engineers, U. S. A., conducted an investigation into the infusorial deposit of Virginia in the Fort Monroe artesian well, which was published after his death in *The Virginias*, October, 1882. After his recovery of health, in 1879, Prof. Rogers contributed to Macfarlane's Geological Railway Guide the chapter on Virginia and West Virginia, containing many valuable original notes.

WILLIAM BARTON ROGERS.

On the 30th of May, 1882, he rose to deliver the diplomas to the graduating class, most of whose course had been passed under his presidency. His voice was at first weak and faltering, but, as was his wont, he gathered inspiration from his theme, and for the moment his voice rang out in its full volume and in those well-remembered, most thrilling tones; then, of a sudden, there was silence in the midst of speech; that stately figure suddenly drooped; the fire died out of that eye, ever so quick to kindle at noble thoughts, and, before one of his attentive listeners had time to suspect the cause, he fell to the platform—instantly dead. All his life he had borne himself most faithfully and heroically, and he died, as so good a knight would surely have wished, in harness, at his post, and in the very part and act of public duty.



MEMOIR
OF
EDWARD TUCKERMAN.
1817-1886.

BY
W. G. FARLOW.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1887.

BIOGRAPHICAL MEMOIR OF EDWARD TUCKERMAN.

The sad duty of presenting an account of the life and writings of our lamented associate, EDWARD TUCKERMAN, has been assigned to the writer by order of the Academy.

Edward Tuckerman, the eldest son of Edward Tuckerman, a merchant of Boston, and Sophia (May) Tuckerman, was born in Boston, December 7, 1817. When a boy he attended Ingraham's school, and later fitted for college at the Boston Latin School. He then entered the Sophomore Class of Union College, Schenectady, and received his Bachelor's degree in 1837. After graduation he entered the Harvard Law School and obtained the degree of LL. B. in 1839. He continued to reside in Cambridge for two years longer, pursuing studies in law and also taking a special course at the Divinity School. In 1841-42 he traveled in Europe, extending his journey as far north as Upsala where he formed the acquaintance of the celebrated Prof. Elias Fries. He returned to this country, it would appear, in the summer of 1842, for in the autumn of that year he made a trip to the White Mountains with Prof. Asa Gray. Soon after, he took up his residence at Union College and there received his Master's degree. He returned again to Cambridge in 1844 or 1845 and, wishing to obtain an Academic degree from Harvard, he entered the Senior Class in 1846 and passed the regular examinations with distinction, receiving the degree of A. B. in 1847. Although he had already obtained two degrees from Harvard, he entered the Harvard Divinity School two or three years later and passed through its course of study and prescribed exercises, although it appears from the Harvard Triennial Catalogue that he did not take the degree of Bachelor of Divinity. It should be said in this connection that his family were members of the Protestant Episcopal Church and he himself remained a member of that denomination during his life, although, at the time of his connection with the Harvard Divinity School, it was under the control of the Unitarian denomination.

In May, 1854, he was married in Boston to Sarah Eliza Sigourney Cushing, who still survives him. Soon after his marriage he removed

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with his wife to Amherst, Mass., where he built a house in a beautiful spot and resided until his death, March 15, 1886. In 1854 he was appointed Lecturer on History in Amherst College, and until 1873 he continued to give instruction in history, during a part of the time filling the chair of Oriental History. In 1858 he was appointed Professor of Botany, a position which he held during the rest of his life, although during his later years he was relieved from class instruction. He received from Amherst the honorary degree of LL. D., was elected a member of this Academy in 1868, and was connected either as an active or honorary member with numerous scientific societies both of this country and of Europe.

As a man, Professor Tuckerman was noted for his sincerity and amiability. He is described by the surviving friends of his earlier years as a most agreeable companion, one whose society was sought by those who prized that good fellowship to which both the heart and head contribute. If he was at times reserved he was also genial when the occasion demanded; if he was often absorbed in his own studies, he unbent when friends sought his society. The writer could repeat many anecdotes told by his old Harvard friends to show how, when students together, they respected his character and enjoyed his companionship. After his removal to Amherst his life was passed in the quiet pursuit of his favorite studies, especially botany, and he rarely left home except to make some botanical excursion. His family relations were most happy, and his wife was not only a devoted companion but also a sympathizer with his work. By his brother professors and, in fact, by the people of Amherst generally, he was highly esteemed as a man and a citizen. As a teacher he inspired the better class of students with an enthusiasm which did not cease with college life, but afterwards developed into an activity of which science in this country has already in part reaped the fruit.

From the first, Professor Tuckerman was of a retiring and sensitive temperament, and, as years passed on, he was forced to become more and more secluded in consequence of a deafness which gradually increased, and at last reached a stage at which conversation became difficult. Although probably never very robust, in his early life he possessed great bodily activity and a degree of venturesomeness and fondness for exploration hardly to have been expected. He made numerous visits to the White Mountains and botanized on the most inaccessible peaks and in the wildest ravines at a time when the White Mountains were as difficult of access as the Rocky

EDWARD TUCKERMAN.

Mountains or the Sierras at the present day. With the exception, perhaps, of Oakes, no botanist has ever explored the mountains with the same zeal and success as Professor Tuckerman, and, as far as lichens are concerned, the collections of Oakes are naturally not to be compared with those of Tuckerman.

His natural activity and power of work were unfortunately diminished by sickness during the latter part of his life. A number of years before his death he suffered from a sunstroke from which he probably never quite recovered, and this made it difficult for him to work continuously as had been his habit. For the last few years he failed steadily, and, in the autumn of 1885, he went to Virginia in the hope of restoring his health; but he soon returned to Amherst and continued in comparatively good health, although he was known to be suffering from Bright's disease, until a few days before his death, when complications of the disease confined him to his room, and he at last passed calmly away.

The writings of Professor Tuckerman, apart from his botanical works, relate to historical and theological subjects, and in the present connection do not call for an extended notice. He privately published an edition of Josselyn's "New England's Rarities Discovered," with annotations, including a biography of Josselyn, and a sketch of the earlier sources of our knowledge of New England plants and of some of the people who made them known. In 1832 and 1833 he assisted Mr. Samuel G. Drake in the preparation of his "Book of the Indians" and "Indian Wars." Between 1834 and 1841 he contributed to the New York Churchman no less than fifty-four articles under the title of "Notitia Literaria" and "Adversaria," upon points in history, biography, and theology. He was also a frequent contributor to other religious journals.

Professor Tuckerman's fondness for botany was shown at an early age. When a student at Union College he was appointed curator of the Museum. His acquaintance with William Oakes, of Ipswich, Mass., one of the pioneers of botany in New England, and Dr. T. W. Harris, a noted entomologist and librarian of Harvard College, dates, perhaps, from a still earlier period; at any rate the influence of these two men—the one an indefatigable collector and explorer, the other a thorough student—must have had much to do in forming his tastes and shaping his future career. Of all plants lichens were always the most attractive to him, and his first paper, "An enumeration of some Lichenes of New England," was presented to the Boston

Society of Natural History in 1838-'39, the year after he graduated at Union College, when he was only twenty-one years of age. This was followed by a second paper in 1840, "A Further Enumeration of some New England Lichenes," and a third in 1841, "Further Notice of some New England Lichenes." These papers, it will be noticed, were the work of a young man who had studied only in this country, and relate to a group of plants which, up to that time, can hardly be said to have been studied at all by American botanists; for, if we except Halsey's "Synoptical View of the Lichens growing in the Vicinity of the city of New York," the references to lichens in works by American botanists consisted of lists of species determined by Europeans from specimens sent to them and published often without the names of authorities, and too frequently with glaring typographical errors. Tuckerman's papers, even the earliest, are full of critical notes on structure and distribution, giving the results of his own explorations, especially in the mountainous regions of New England, whose lichen-flora he was the first to investigate.

The most important event of his life, botanically considered, was his journey to Sweden in 1841, where he met Elias Fries, professor of botany at Upsala, the leading lichenologist of his time, and, after Linnaeus, the most distinguished of Sweden's many distinguished botanists. Thirty years later, when the writer was at Upsala, Professor Fries, then a venerable man of eighty with undiminished mental vigor, recalled the days when the enthusiastic young American was at Upsala and related how, when walking together on the famous avenue near the university, Tuckerman discovered a species of lichen which he, the authority on lichens, had not seen there before. The visit to Fries was important because it enabled Tuckerman to acquire, if one may say so, the traditions of the science. In some branches of cryptogamic botany it is almost a necessity that an American should see the species of Europe under the guidance of a botanist trained on the spot if he would clearly recognize the same species when they occur in America. There is an indescribable something, especially in lichens, which certainly is not and probably could not be laid down in books. It is fortunate for our lichenologists that Tuckerman was able to transfer to America and perpetuate on this side of the Atlantic the ideas of classification and specific limitations derived from Fries himself. Certainly

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during his life he always adhered to the Friesian views of classification, which he preferred to those of later botanists.

While in Europe he did not limit his botanical studies to lichens, but also worked on some of the more difficult genera of phænogams. Before his return to America he contributed to Hooker's Journal of Botany a paper on "*Oakesia*, a new genus of *Empetreae*," and shortly after his return, in 1843, he printed privately at Schenectady his "*Enumeratio Methodica Caricum Quarundam*," of which Professor Gray says: "He displayed not only his critical knowledge of the large and difficult genus *Carex*, but also his genius as a systematizer, for this essay was the first considerable and a really successful attempt to combine the species of this genus into natural groups." In the American Journal of Science of the same year he published "Observations on some interesting plants of New England," and this was followed in 1848 and 1849 by two papers on similar subjects, including an elaboration of the American species of the difficult genus *Potamogeton*, which he was the first in this country to study critically. These papers include about all that was ever published by Professor Tuckerman on phænogamic plants, if we except the "Catalogue of plants growing without cultivation within thirty miles of Amherst College," issued in 1875, of which he prepared the list of flowering plants. But it should be added that the papers already named do not comprise all that he contributed to phænogamic botany, for he furnished valuable notes on distribution of species to other workers in the same field, which, if they were sometimes properly acknowledged, were, it is to be feared, sometimes absorbed without suitable recognition.

In 1845 he published in the Journal of the Boston Society of Natural History "A further notice of some alpine and other lichenes of New England," and in the same year there appeared at Cambridge his "Enumeration of North American Lichenes, preceded by a general account of lichens and of the Friesian system, together with an essay on the natural systems of Oken, Fries, and Endlicher." In 1847 he presented to the American Academy of Arts and Sciences a "Synopsis of the Lichenes of New England, the other Northern States, and British America," which was issued separately the following year. This work was the first attempt at a systematic description and classification of all lichens known, at that time, in the temperate regions of North America and may be called the lichen-primer of this country. It included 295 species, of which 20

were new. Incomplete as it seems at the present time, it offered to the student of that day the means of recognizing and referring to their proper places in the then accepted system the more prominent species of the eastern portions of the United States and it served as an incentive to the study of lichens which was important. As an adjunct to the synopsis should be mentioned the "*Lichenes Americæ Septentrionalis Exsiccati*," in three fasciculi, Cambridge, 1847-1855, including specimens of many of the species given in the synopsis and collected mainly by Tuckerman himself.

But the study of lichens soon assumed a new phase. The microscopic characters began to be more carefully studied, new regions were explored, and the number of observers increased rapidly. Naturally, with the increase of collections and a more minute anatomical study, not only did the older descriptive works prove inadequate but the systems of classification themselves required modification. The Western explorers and a new generation of botanists in the East amassed a large amount of material, while the various expeditions to foreign lands brought home rich collections of lichens which were placed in his hands for determination. From 1848 to 1872 he published numerous papers of which the more important were a "Supplement to an Enumeration of North American Lichens," in the *Journal of Science* in 1858 and 1859, where he described many new species from the Southern States and California; *Observationes Lichenologicæ*, in four parts, in the *Proceedings of the American Academy*, 1860 to 1877; the *Lichens of the Wilkes Exploring Expedition*, and the *Lichens of California, Oregon, and the Rocky Mountains*; Amherst, 1866. The second and third parts of the *Observationes* related principally to species collected in Cuba, by Charles Wright, and the range of his studies of exotic forms is shown by the fact that, not including the papers previously named which contained accounts of species from Eastern Asia, the Pacific Islands, the Cape of Good Hope, etc., he also published papers on species from Kerguelen's Land, the Hawaiian Islands, and Arctic America.

The elaboration of this rich material enlarged his views of classification and gave him a broad knowledge of generic types as well as specific forms which he embodied in his "*Genera Lichenum*," published at Amherst in 1872. This, it seems to the writer, should be regarded as his greatest work. In it he displays a remarkable knowledge of lichen forms and a thorough acquaintance with the

literature of systematic lichenology. That it is not more widely read and known is probably due to the prevailing fondness for microscopic details. It must be confessed, however, that the somewhat involved style in which it was written is in part responsible, for it is a hard book to read. It was addressed to experts, not to beginners, and, when it appeared, the older experts were too busy attempting to split up genera and species to an unendurable degree of artificiality, while the younger men, attracted by the writings of Schwendener, Bornet, and Stahl, were too much interested in developmental and physiological questions to care much for systematic works. The *Genera Lichenum* is a protest against the artificial classifications based almost wholly on the spore characters without regard to other equally important characters, a method first advocated by the Italian lichenologists, with De Notaris at their head, and adopted by the Germans and other continental botanists. Tuckerman advocated the systems of Fries modified by his knowledge of exotic forms. His view of species was a large one, and he recognized numerous varieties of the type but refused to admit that, if a form differed in any visible respect from the type, it must constitute a distinct species. If his classification is at times less easy to follow than that of the modern continental school, it presents ultimately fewer difficulties and is certainly more natural, and hence more scientific.

On the completion of his *Genera* his whole aim was to complete a descriptive work which should include all the species known in the United States. For this work no living botanist had so ripe an experience or such a mass of valuable material as he. Unfortunately his health failed, and, being the long acknowledged authority on lichens in this country, much of his time was absorbed by correspondents who, at times, ill requited his valuable services. In 1882 there appeared the first volume of his much-desired "*Synopsis of the North American Lichens*," which comprised the *Parmeliacei*, *Cladonie*, and *Cænogonie*. He applied himself industriously to the preparation of the second volume, which it was hoped would soon appear; but death interrupted his task, and it is not now known whether his manuscript is in a state to admit publication.* If not, the loss to American botany will be very great, for

*Since the above was written it has been learned that the manuscript of the *Lecidiacei* and *Graphulacei* was left by Professor Tuckerman in condition for printing, and it is the intention of his family to publish it at an early day.

it is certain that he had in his possession a large amount of new material, and no one could so well as he treat the difficult groups *Lecidiacei* and *Verrucariacei*.

A word should be said about Professor Tuckerman's views with regard to the Schwendener theory. Following the earlier indications of De Bary, Schwendener, by a minute study of the thallus of lichens, brought forward proofs to show that the gonidia of lichens had no real genetic connection with the hyphæ, but were, in fact, algæ upon which the hyphæ were parasitic; in other words, what is called a lichen is, properly speaking, a fungus of the order *Ascomycetes*, which is parasitic on some alga; in most cases a species belonging either to the *Parmellaceæ* or the *Nostocaceæ*. The botanical world was divided in opinion, and, for the last fifteen years, the so-called algo-fungal theory of lichens has given rise to endless controversies of a personal and very acrimonious character. As a rule, the systematic lichenologists were opposed to the theory, while the histologists supported it. Professor Tuckerman, who began his studies at a time when microscopic technique had not reached its present perfection, could hardly have been expected to take a very active part in a direction requiring difficult microscopical work. It is said that at first he was inclined to favor the theory, but if so, he soon changed his views and sided with the opponents of the theory. It must be said to his credit that his references to the subject were always courteous and dignified in marked contrast with the course of some other well-known lichenologists. The most definite expression of his opinion in print is to be found in the *American Journal of Science and Arts* of February, 1879, where, under the title of "The Question of the Gonidia of Lichens," he reviewed the first part of Minks' "Beitraege zur Kenntniss des Baues und Lebens der Flechten." Minks had observed small green bodies in the hyphæ, which finally escaped from them and developed into gonidia. In his review Tuckerman confirms the statement of Minks with regard to the existence of the green bodies called by Minks microgonidia, and remarks that he has observed them himself in *Parmelia tiliacea* v. *flavicans* of Cuba, but he does not go so far as to say that he saw them develop into gonidia. He accepts Minks' statements on this point, however.

The life of our lamented associate was one devoid of some of the incidents which make the lives of many men of science picturesque. It is a simple story, not of struggles against poverty and the enmi-

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ties of opponents, but of one who, endowed with a fair share of this world's goods and always happy in his family relations, pushed steadily onward in his chosen field—a worker to the last in spite of increasing bodily infirmities. If it was a life so quiet and retired that it may almost be said to have been eventless, it none the less has left its mark. Professor Tuckerman's death has left a gap in the ranks of American botanists which will not be filled for years, and while his contemporaries mourn the loss of a personal friend, the younger generation of botanists feel no less keenly the loss of one whose letters were not only highly instructive and encouraging, but also full of enthusiasm and enlivened by reminiscences of the time when he was young.

The name of Tuckerman is commemorated in a genus of *Compositæ*, dedicated to him by Nuttall, and in several species named in his honor by different botanists. A more widely known memorial is the noble ravine on Mount Washington, the seat of his early explorations, which now bears his name. He will long be remembered by the poor and afflicted relieved by his sympathy and beneficence whose extent, not suspected even by his intimate friends during his life, has been made known only since his death by the grateful recipients of his bounty.

LIST OF THE BOTANICAL WRITINGS OF EDWARD TUCKERMAN.

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MEMOIR
OF
EDWARD B. HUNT.
1822-1863.

BY
F. A. P. BARNARD.

READ BEFORE THE NATIONAL ACADEMY, AUGUST, 1864.

BIOGRAPHICAL MEMOIR OF EDWARD B. HUNT.

When a man distinguished above his fellows for mental or moral excellence passes away at length from the scene of his usefulness on earth the sorrow with which friendship cherishes the remembrance of his worth is shared by a wide circle of those to whom he was only known by his honorable repute among men. In such cases it may afford a species of sad consolation, if it can be said of the departed that he had filled up the full measure of the years ordinarily allotted to man, and had been permitted to bring to perfection the labors to which his life had been devoted. If, on the other hand, he has been removed in the flower of a vigorous manhood, and in the full energy of a ripened intellect, leaving behind him rather the proofs of noble capabilities than the accomplished results which they seemed destined to achieve, the grief which accompanies the recollection of what he was is greatly enhanced by the thought of what he might have been. It is with such feelings that we must cherish the memory of the gifted subject of the following imperfect sketch.

Edward B. Hunt was born in the town of Portage, Livingston county, New York, on the 15th day of June, 1822, the youngest of a family of nine children. In his early childhood he evinced, to a marked degree, the characteristics which chiefly distinguished him throughout his life. He partook but very slightly of the ordinary fondness of childhood for amusements. Indeed, it is said of him by those who were his companions at that period that he was never known to play, but that to work, of whatever description, he was ever ready to address himself with the greatest alacrity. His remarkable physical strength made him, even when very young, most efficient in all the tasks which employ the hands, and admirably fitted him to be useful amid the exigencies of a somewhat pioneer life; but it was in study that he found his chief interest, even from the first dawning of intelligence. Study had for him an irresistible fascination, and to this he constantly turned with avidity in all the intervals of more active employment. Indeed, it is the testimony of one whose opportunities for knowing him were probably better than those of any other that "work, either with head or with his

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hands, seemed to be an absolute necessity of his nature," and that "the lack of a capacity for recreation, or even for a remission of labor, was in after years a serious deficiency in his temperament, and one over which he often mourned."

His early opportunities for education were by no means considerable. His rudimentary education seems, indeed, to have been almost entirely self-acquired. For a limited period—it is thought not more than two or three academic terms—he was a student in one of the village academies of his native State; but so diligently had he employed all his available hours of leisure previously to that time, though almost entirely without assistance, that he at once, and very easily, took the lead of all his school-mates.

In the year 1841 young Hunt received an appointment to the United States Military Academy, and imperfect as had been his means of preparation beyond those which his own native resources supplied, he was found at his entrance to be well qualified for admission. In a class conspicuous for talent he, from the outset, took a high position, which he maintained throughout his academic career. At his graduation he stood second upon the roll of merit, having closely contested the highest position with his friend Whiting, now in the military service of the insurgent government, who always generously confessed that the order should have been reversed. The difference between the competitors seems to have been that, in solidity of attainment and strength of mind, the superiority was with Hunt, but that Whiting possessed the advantage in the readiness and facility with which he was able to display his acquisitions. At his graduation Mr. Hunt was recommended to the Department of War by the academic board of the institution for a commission in the Corps of Engineers, an honor at that period conferred on those who had given indisputable evidence of high talents, superior attainments in study, and moral worth. During three years shortly after his graduation, from 1846 to 1849, he was stationed at West Point, as one of the assistant professors, giving instruction to the Department of Engineering. In this situation he not only discharged his duties with ability, and to the entire satisfaction of the able professor then and still at the head of that department, but he at the same time carried on a great amount of professional and other reading with a profit which made itself very manifest in his subsequent life.

In 1851 Mr. Hunt, then Lieutenant Hunt, was assigned to duty

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in connection with the United States Coast Survey. In this service he continued until 1858, being generally attached to the office at Washington, and especially charged with the superintendency of the engraving of the charts. His active mind, however, did not permit him to confine his attention to his immediate duty; but he availed himself of the advantage his situation afforded him to familiarize himself with all the details, theoretical and practical, of the methods employed in conducting that great national work. The annual reports of the survey, during the time of his connection with it and later, contain evidences of his industry in the form of contributions upon various subjects, embracing Lithography and Lithographic Transfers, Map Projections, with accompanying tables, Engraving in Relation to the Coast Survey, Description of the Base-Measuring Apparatus, Description of the Self-Registering Tide Gauge, Notes on a new Sounding Apparatus, proposed by him for deep-sea sounding, &c. In the report for 1854 is also embraced an index of the reports for the ten years from 1844 to 1853, inclusive, prepared by Major Hunt with great care. He indexed also the reports for 1851, 1852, 1853, and 1854 separately, and prepared an index of all the sketches published by the office for the ten years preceding 1854.

The Transactions of the American Association for the Advancement of Science, and the volumes of the American Journal of Science from 1850 to 1863, contain many papers from his hand, some of them of very noticeable interest. His earliest published paper, so far as the writer has been able to discover, appeared in the Journal of Science in 1850, and relates to the interpretation of Marriotte's law of gaseous pressure. It is worthy of an attention greater than its extent might seem to challenge for it as embracing the germ of a theory of molecular physics, which became subsequently the favorite study of his life, and of the importance of which, as presenting the true and the only true interpretation of nature, he entertained the most undoubting conviction. In this paper he aims to show that Marriotte's law must necessarily be true of an elastic medium entirely homogeneous, no matter what be the law of forces by which the parts of the medium act upon each other, and, accordingly, that, since Marriotte's law is not absolutely true of any known gas, therefore the homogeneity of every such gas must in point of fact be in some way interrupted. Such an interruption may be conceived by supposing a laminated, filamentary, or

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molecular structure. Either of the two former hypotheses would confer special properties in determinate directions. Such properties not being found in gases, it follows, as the only possible conclusion, that their constitution must be molecular.

In the year 1854 Major Hunt published a more extended paper (also in the *Journal of Science*) entitled "The Nature of Forces," in which his peculiar ideas on the forces actuating the molecules of matter are more distinctly brought out than in any other of his printed productions. No analysis of this article will be attempted here, but it may be mentioned, in illustration of its boldness, that it claims to explode, as a monstrous and absurd supposition, the plausible and still favorite theory of Boscovich upon the same subject.

In 1855 Major Hunt presented a paper to the American Association recommending the construction of an index of scientific reference on the plan of the well-known index of Dr. Young, but brought down through the period of more than half a century, exceedingly fertile in scientific publications, to the present time. This was a subject which had early occupied the attention of Professor Henry, and on which he had communicated with the British Association. They in turn had presented the subject to the Royal Society, who gave it a favorable consideration. The undertaking has never yet been completed in its full extent. The design was to prepare an index to all the papers of scientific interest, which are scattered through journals of every kind, and the transactions of scientific bodies, leaving systematic treatises on similar subjects, as at present, to be sought for in proper bibliographies.

For the use of the Coast Survey, with which he was then connected, Major Hunt undertook the great labor of preparing by himself alone an index on the principle above mentioned, of papers having a relation to that great work. The amount of labor performed by him in the execution of this task may be understood from the statement in the Coast Survey report for 1857 that at that time there had been examined by him seven thousand volumes from which had been extracted thirty-five thousand titles. This work had been done by him within a period of about a year, in addition to his regular daily duties. Before the entire completion of this undertaking Major Hunt was ordered to Florida on engineering employment, there remaining still to be examined about one thousand volumes.

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Of the other published papers of Major Hunt the following appear to be the most important:

The Nature of Organic Structure.

The proposed Geographical Survey of the State of New York.

A proposed Trigonometrical Survey of the State of New York.

The Conical Condenser—a proposed attachment of annular conical reflectors to concentrate a large beam of light within the aperture of an ordinary telescope.

Project of a Geographical Department of the Library of Congress.

On our Sense of the Vertical and Horizontal.

On Infinity—Physical and Metaphysical.

On Scientific Publication—its Ordinary Practice by Associations, which the author holds to amount, in general, to no publication at all.

Physical Notes at Key West.

The use of Air as a Medium of Conveying Mechanical Power, published in 1851, and anticipating in theory what has been actually reduced to practice in tunnelling the Alps by the Sardinian Government.

Cohesion of Fluids, Evaporating, and Steam-Boiler Explosion.

In the year 1857 Major Hunt was assigned to duty with the Corps of Engineers to which he belonged, and for the three or four years succeeding was stationed at Fort Taylor, Key West. That this period of comparative isolation from the world was diligently employed by him in the prosecution, so far as his professional duties would allow, of his own independent studies and investigations in science is evidenced by the number and value of his contributions to the scientific journals and to the proceedings of the American Association for the Advancement of Science. The reports of the Coast Survey continued also to be enriched by occasional papers from his hand on subjects suggested by the peculiarities of his geographical position, as, in 1858, On the Dynamics of Ocean Currents, On some Anomalies in the Florida Gulf Stream, and, in 1862, On the Origin, Growth, Substructure, and Chronology of the Florida Reef.

In the year 1862 Major Hunt was relieved from duty at his monotonous Florida post, and ordered to New York. Some time not long before this occurrence he had conceived the idea of an invention designed to introduce a novel and, in the event of its success, a fearfully destructive means of offense in ocean conflicts. In letters to his private correspondents before he left Key West he spoke of this device in terms of most enthusiastic confidence, expressing his conviction that it was destined entirely to revolutionize the science

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of naval warfare. After his return he devoted himself, with his earliest opportunities, to a series of experiments, conducted, as his official engagements permitted, with a view to test the practicability of the invention, and to verify the truth of his anticipations regarding it. It was in the prosecution of one of these, as is already generally and too sadly known, that this admirable man and accomplished physicist paid with his life the price of his devotion to the cause of science and of his country; for it was first and above all other things for his country's sake that he desired to see this new and formidable engine of warfare perfected, and he entertained no shadow of a doubt that the nation which should be the sole possessor of the secret of its construction would be absolutely supreme upon the ocean. The design of the contrivance may be stated in general terms to have been to direct a missile beneath the surface of the water against a distant object with destructive effect. Perhaps it might be inexpedient to make public at present a more particular description. Fortunately the materials for such a description exist, and the views of Major Hunt as to the course of experimentation proper to be pursued in working out the project to completion had been so fully imparted to scientific friends before the occurrence of the deplorable accident which put an end to his life that there could be no difficulty in taking up the investigation where he left it. Considering the great practical importance of the object aimed at in this invention, its apparent feasibility as a project, and the degree of actual success which attended the earliest experimental trial—a rather imperfect one—of its merits, it would seem that some measure ought to be taken to test thoroughly its capabilities. Considering, further, that, if proved to possess the character which its inventor ascribed to it, it is of a nature to be useless to an individual, so that the resulting advantage would accrue to the country exclusively, there can be little hesitation in saying that the further prosecution of the experimental investigation commenced by Major Hunt ought to be undertaken by the Government and continued until every doubtful point connected with it is satisfactorily settled.

The name given to his contrivance by Major Hunt was the "Sea Miner." It was not, as might possibly be inferred from what has thus far been said, simply a gun of ordinary description fired under water, or a contrivance to facilitate firing a gun in such a situation. That a gun may be so fired without bursting, and may expel a pro-

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jectile with considerable velocity, has been proved by recent experiments, but it has not been proved that such projectiles can be effective unless at very short distances. It was the design of the "Sea Miner" to enable a ship of war to destroy its antagonist at distances not much, if at all, less than cannon-shot range in the air. The earliest experiments were intended merely to test the correctness of the general principles involved, and the very first one of these was, in this point of view, esteemed by Major Hunt a success. The projectile was driven about 150 feet under water, when it emerged from the surface and passed in a curve about 150 feet more, falling again into the water, after which its progress seems not to have been traced; but the experiment which satisfied the inventor of the feasibility of his project developed also some mechanical difficulties requiring attention, and other similar difficulties made their appearance in subsequent trials. In this respect the history is that of all important inventions, of especially all which are distinguished for originality. Major Hunt saw nothing in them in the least degree formidable. A little time and a careful variation of the conditions of the successive trials were all that he esteemed necessary to give him entire mastery of the subject, and to make the results sure. Unfortunately this time was not allowed him. He can hardly be said to have fairly made a beginning before the occurrence of the accident already mentioned, which brought at once to a close his labors and his life. The nature of the accident may be briefly described. In order to direct a projectile horizontally through the water, it was necessary that it should be discharged from a point beneath the surface. The hold of any ordinary vessel of sufficient draught, if properly pierced through the side, would have answered the purpose, but, from considerations of economy and convenience, Major Hunt employed a partially submerged floating water-tight box. This was covered with a deck, an aperture or "man-hole" being left in the top, through which the submerged chamber might be entered by means of a ladder.

In the ordinary and proper action of the apparatus the gases evolved at the expulsion of the projectile from the barrel in which it was inclosed were effectually excluded from the chamber, but in the present instance, through some unaccountable oversight, the usual provision had not been made for their egress, and the pressure accumulated until the joints on the side of the chamber were forced open and the chamber itself filled with the noxious products of

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combustion. Of this condition of things Major Hunt was not aware. He was not within the chamber at the time, but descended directly after and was seen by his assistant to return almost immediately to the ladder in great haste. He began instantly to ascend and had reached a height of about ten feet from the floor, when his strength entirely failed him and he fell heavily to the bottom. Two or three men sprang down successively to his aid, but each was immediately prostrated in the same manner. Water was then profusely thrown down by those above, by which the noxious fumes were partially absorbed or dispelled, and one or two of the men were able to help themselves out. The others, with Major Hunt, were removed as quickly as possible, and all except him soon recovered. Major Hunt, however, remained insensible, though living, for a period of forty-eight hours, when he finally ceased to breathe. It was the unanimous opinion of the physicians who attended him that the immediate cause of his death was the concussion of the brain produced by his fall.

Thus suddenly and prematurely passed away one of the most promising young physicists whom the country has produced, and one of the most conscientious and faithful of the officers in its military service—a man who was, at the same time, in every domestic and social relation in the very highest degree estimable and admirable.

In forming an estimate of Major Hunt as an intellectual man and man of science, we must take into consideration not so much the amount which he actually gave to the public as the evidence it displays of earnest and original thought, and the promise it holds out of higher achievements in the future. That these characteristics are apparent in most of his published papers will be generally admitted. We must take further into consideration the variety and number of literary projects left behind him incomplete, and his large accumulation of material in the form of notes and manuscripts for future use. These embrace almost every subject of interest throughout the entire range of physics, and furnish striking proof of the extent of his reading and study. Similar evidence appeared in his conversation. Hardly a topic could be introduced on which he was unprovided with something interesting in the way of information.

His mental habits led him continually to speculate upon causes and first principles, somewhat to the neglect or disregard of prac-

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tical applications or results in detail. He was in danger, therefore, of being considered by some as rather fanciful than substantial, yet there were few of his speculations which would not bear examination, or which did not acquire importance in proportion as they were considered. Among these, that in which he himself felt the deepest interest, was, as has been mentioned already, his doctrine of molecular physics. Unfortunately he has left nowhere an adequate account of this, and we are unable to judge of it from a full understanding of its merits, but he undoubtedly himself felt that, in this field, he had struck an original vein, which was in his own view so important—so even grand in its promise—that (as is said by one who knew him best) he often questioned whether it were not his duty to lay everything else aside that he might make its elaboration his life's work. When he was engaged in one of these trains of thought it is further remarked of him that he lost all consciousness of the lapse of time, and often spent a whole night in concentrated thought upon some point that baffled him; and it is added that it was after one such night of wrestling that he told his family that he had at last succeeded in clearing up a difficulty which had embarrassed him for years. It was a constant and often most intolerable trial to him not to be able to devote himself wholly to this study. It was the only thing in regard to which he ever used the language of extravagant or exaggerated enthusiasm; but of this he often said, in the confidence of his family, that it was the only thing on earth which he believed he could do especially well; that he felt drawn to it as to a mission, and that it seemed cruel that the urgent claims of his profession, and the necessity that he should have a profession, should debar him from doing his appropriate work. It is, indeed, deeply to be regretted that the world should have been deprived of the results which might have followed from the devotion of such a mind, animated by so fervent a zeal, and by so profound a conviction of the correctness of the fundamental principles assumed in the inquiry to the complete development of the theory which he had so much at heart—a theory upon a branch of physics which is still far from being in a satisfactory condition.

As a man, Major Hunt's most distinguishing characteristics were integrity of purpose and purity of thought. His dislike of everything paltry, mean, or false—nay even of everything vulgar or coarse—was so instinctive, so strong, and so unconcealed as almost to constitute a barrier between him and those in whom such traits

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appeared, and sometimes, perhaps, to lead to an antagonism which he was not worldly enough to dissemble. He was, nevertheless, exceedingly averse to contention and strife. Petty quarrels or personal bickerings he could not endure, and as a necessary consequence of this feeling and of his native goodness of heart he was remarkable for his universal courtesy, and for the pains he took to conciliate the good will of all whom he met, and to contribute to the happiness of those with whom he was associated. As a friend, he was exceedingly generous, no amount of trouble or self-denial being esteemed by him too great when he felt that he might do a service to one whom he esteemed. He commanded thus the entire confidence of those who intimately knew him, and the circle of his friends was almost coextensive with that of his acquaintance.

In regard to his religious character I cannot do better than to quote the testimony of the witness who saw him most constantly in the complete unreserve of his most private life. "He was not," says Mrs. Hunt, "what would be called, in common speech of the common religious world, a religious man. He had never attached himself to any church organization, and he had a great dislike of their narrowing effects on the mind; but I never knew him to retire without reading a chapter in the Bible, and it is my deep and honest conviction that he had never in his whole life done a deed which he regarded as wrong. Those who best knew him have often felt that his peculiar and native integrity made it, humanly speaking, impossible for him to do anything wrong. This peculiar quality of natural, unconscious, and indomitable uprightness is a very noticeable trait in his whole family of sisters and brothers. It was to this unyielding sense of right that his life was at last the sacrifice. No one but myself can ever know at how great a cost of self-relinquishment it was that he consented, during the months and years of this war, to remain out of the field, out of the line of promotion, and much of the time liable to the misconstructions and suspicions of persons who did not understand what his duties really were; but he felt that, in the field, he could be but one man more, and could do little for the country, while, by perfecting his submarine invention, he could do incalculable good. The thought of any benefit to himself from the success of the invention never presented itself to him. I shall never forget the positively terrible and contemptuous severity with which he replied to a well-meant suggestion from a friend that he should complete the 'Sea Miner' himself and sell it

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to the Government. When I reflect on his utter unselfishness in regard to it, and on his lost life for its sake, I cannot but feel that the Government owes it to him to make every effort to complete and perfect his work."

With this extract the present imperfect sketch may suitably terminate. Those who knew Major Hunt will recognize in what has been written some not unfamiliar outlines, but those who knew him well will easily discover how far it fails to do him justice.

MEMOIR
OF
JAMES CRAIG WATSON.
1838-1880.

BY
GEORGE C. COMSTOCK.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1888.

BIOGRAPHICAL MEMOIR OF JAMES CRAIG WATSON.

At some unknown time prior to the war of revolution the ancestors of James Craig Watson emigrated from Ireland to the colony of Pennsylvania. We know little of the fortunes of this family before the early years of the present century, but they must at that time have been at a low ebb, for in 1811 we find James Watson, the grandfather of the future astronomer, abandoning the land of his birth and pushing westward seven hundred miles to build for himself a new home in the almost unbroken forests of Upper Canada. The journey was made on foot. William, the father of James Craig Watson, then an infant of tender years, was placed upon an ox-sled which, slipping easily over the fallen leaves, bore the scanty household goods of the family.

They reached their destination in safety, and the work of clearing and cultivating was begun, but the times were unpropitious. Before a year had passed war was declared between the United States and Great Britain, and, Canada becoming the scene of hostilities, James Watson was compelled to abandon his farm for a time and to fly to the east for safety. Upon the return of peace he again took up his former residence, prospered in his labors, and was at the time of his death a man of wealth and consequence in the community which had grown up about him. But this man's life was not devoted wholly to clearing up his homestead and increasing his acres; he possessed a taste for books and for learning, for the gratification of which he collected an excellent library whose appreciation and use he taught to his children.

Amid these surroundings William Watson grew to manhood, acquiring his father's tastes and learning, but not his energy and practical ability. At the age of twenty-six he married Rebecca Bacon, a native of Nova Scotia. Four children were the fruit of this union, of whom the oldest, James Craig Watson, was born near the village of Fingal, county of Elgin, Canada West, on January 28, 1838. The boy inherited a genial disposition and the taste for study which we have noted in his father and grandfather, but the restless and tireless activity which was displayed throughout his whole life was clearly derived from his mother.

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The first twelve years of his life were spent upon the farm in Canada, his father pursuing by turns the occupations of farmer, carpenter, and schoolmaster, and instructing his children, three sons and a daughter, in that measure of book lore which he himself had acquired. But affairs went ill with William Watson, and in 1850 he found himself compelled to abandon his home and seek elsewhere a livelihood for himself and his family. He turned westward, not knowing where his foot should find a resting-place, but resolved to leave Canada. Arrived in Detroit, Mich., the family found its choice of a home limited to the villages along the single line of railway that then extended west a scant hundred miles from Detroit. A chance remark made by a stranger that the State University was situated at Ann Arbor on this line of railway, only forty miles distant, determined the choice. A vague idea that perhaps her children might derive some educational advantages from it seems to have been in the mind of Rebecca Watson, and the decision was made by her.

The family reached Ann Arbor penniless and more destitute than that ancestor who, forty years before, had emigrated to the frontier of Canada. The father found work in a small factory, and James, now a bright boy of twelve years, was employed in various menial offices at the same place. The following years were years of bitter poverty and want, and they left their imprint deep upon the after life of James Watson; but they were also years of development. He quickly learned the work of the factory and noted the incompetence and faithlessness of the man in charge of the steam-engine, which he reported to his employer. "I know, Jimmy, the man ain't worth his salt, but he is the only one I can get for the job." "I can run the engine, sir," was the reply. "Oh, no, Jimmy, you don't know how to manage an engine." "But I do;" and the dispute was settled by going to the engine-room, where Jimmy satisfied his employer that he did understand the engine. The incompetent man was forthwith discharged and the boy of thirteen became engineer of the factory.

Here he had some chance for study, and a Latin or Greek textbook was kept in a convenient corner and brought out for use whenever a few minutes could be spared from work. In the following winter the factory was closed and Watson was reduced to the necessity of peddling apples and books at the railway station, an occupation which was exceedingly distasteful to him. As spring

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approached he disappeared, and for a fortnight nothing was heard from him, until his former employer, who had become much interested in the boy, found him in Detroit, where he had made a bargain with the master of a vessel to go sailing with him in the lake trade, and was to have started on the following day. He was induced to return home, and soon after it was resolved that he should attend school, but at the end of the first half day the relations of teacher and pupil were interchanged; the boy withdrew from school, and the master came to him for instruction in algebra and geometry.

During the next two years Watson's employment was of a desultory character. There were vague hopes that he might at some time enter the University, and he, therefore, pursued by himself the study of Latin and Greek; but he also worked successively at nearly all the trades that were practiced in the village, became a competent workman at most of them, and acquired special skill as a machinist. It was noticeable that during this period he avoided association with boys of his own age and sought the companionship of men, with whom he would discuss topics of current interest, entertaining them with jests and with exhibitions of expert penmanship or of skill in arithmetical operations.

At the age of fifteen Watson entered the University of Michigan as a student, and soon attracted attention for the excellence of his scholarship in every direction. For the classical languages he displayed unusual capacity, and his now venerable instructor, Dr. Frieze, bears witness that "his facility in translation was greater than that of most professors of Latin and Greek;" and the writer of this sketch well remembers the enthusiasm with which he was wont in his later years to refer to his study of the ancient tongues.

The man who, during Watson's university career, exercised the greatest influence over his development was, probably, Francis Brünnow, who had been recently called to the chair of astronomy and the directorship of the new observatory. Brünnow introduced into a western college the methods of a German university, and lectured in broken English to despairing and dwindling classes until Watson remained as his only pupil. He became interested in astronomy early in his college course, and in his junior year began work in the observatory. The theoretical and practical sides of the science seemed to have equal charms for him, and his attention was divided between the *Mécanique Céleste* and the construction

of a refracting telescope. His mechanical talent and the training of the machine shop triumphed over the difficulties of the latter and produced an excellent instrument of four inches aperture, the grinding and polishing of the glass and the construction of the mounting being all done by his own hands. Nor was this work of a purely empirical character. There exists now, in Watson's handwriting, a long manuscript translation from *Precht's Dioptrik* upon the theory and construction of achromatic objectives and bearing at its close the signature and date, James C. Watson, Jan'y 8, 1857. He was then 19 years of age.

Watson graduated in 1857, and almost immediately thereafter commenced work in the observatory as a salaried assistant. His earliest contribution to a scientific periodical which I have been able to find appears in Vol. V of Gould's *Astronomical Journal* and bears date April 20, 1857. His great activity at this time is shown by the index to this same volume, which contains under his name the titles of no less than fifteen papers, all published before he had completed his twentieth year.

In 1859 Brünnow resigned his chair in the University of Michigan to assume the directorship of the Dudley Observatory, and Watson was elected Professor of Astronomy and took charge of the observatory, but without the title of director. In 1860 Brünnow returned to Ann Arbor and was re-elected to his former position, Watson being transferred to the chair of physics, which he held until 1863, when, Brünnow again resigning, he was chosen to be Professor of Astronomy and Director of the Observatory. The records of the Board of Regents of the university show that this appointment was made upon the recommendations of B. A. Gould, Elias Loomis, William Chauvenet, Benjamin Peirce, Joseph Winlock, J. M. Gilliss, and others. In the manly communication to the governing board of the university, in which Watson presents his application for appointment to the directorship and sets forth his qualifications for the position, he states that, in addition to the discharge of the duties which his instructorship in the university entailed upon him, he had prepared and published thirty-two original papers upon astronomical subjects. An examination of his contributions to the periodicals shows that this activity was mainly expended in work upon comets and the minor planets. There are numerous communications containing observations and computations of orbits and ephemerides, and an occasional paper of a theo-

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retical character upon the determination of orbits. To this period of his life belongs his "Treatise on Comets," a work of a popular character, to whose preparation he was incited by the interest aroused by the great comet of 1858.

In May, 1860, he married Annette Waite, of Dexter, Michigan, who, during the remaining twenty years of his life, maintained a constant interest and partial co-operation in his scientific work. No children were born to them.

Early in the '60's Watson became interested in the reduction of the Washington Zones, which had been undertaken by Dr. Gould, and for several years a considerable part of his time was given to computations upon this work. As a computer he possessed extraordinary skill and rapidity, attested at a later period by the computation of elliptic elements of a planet's orbit at a single sitting. The possession of this skill perhaps acted injuriously upon the character of his scientific work, as it led him to give much of his time, as a paid computer, to work which others of inferior talent could have done equally well, though less rapidly.

Immediately after his appointment to the directorship of the observatory Watson began the preparation of a series of charts of stars lying near the ecliptic, and his reports upon the work of the observatory during the following ten years represent this as being his principal employment, and dwell upon its laborious character. But scant traces of this work can now be found in the records of the Ann Arbor Observatory. The charts which he prepared have become the property of the National Academy of Sciences and are deposited at the Washburn Observatory of the University of Wisconsin. Of these charts, nineteen in number, only two are finished. They are in their general plan similar to Peters's well known charts, but are far from being equally complete. The discovery of the minor planets, with which Watson's name is associated, was a direct consequence of this work; indeed the expectation of such discoveries was probably the incentive to it. His first planet, Eurynome (79), was found in 1863, only three weeks after his election to the directorship of the observatory. Four years elapsed before another was found, but during the ensuing years they came in rapid succession. In all, twenty-two of these bodies were discovered by him, the year 1868 alone contributing six to the list, at that time an unprecedented feat. The Lalande prize, decreed to him July 11th,

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1870, by the *Académie des Sciences*, was the reward of his earlier labors in this field.

The composition of his treatise upon *Theoretical Astronomy* belongs to the earlier years of his directorship and stands in close relation with his work as a computer and observer of the minor planets. The completion of this treatise, which appeared when he was but thirty years of age, left Watson's hands free for other work, and in 1869 he became associated with Benjamin Peirce in work upon the improvement of the lunar tables. For five years he was engaged in a comparison of the theories of Hansen and Peirce with observation, and spent much time in the endeavor to simplify Hansen's tables, with results which, though satisfactory to himself, were never published and are now lost.

The eclipse expeditions to Iowa in 1869 and to Sicily in 1870 interrupted the continuity of this work, and in 1874, when he accepted charge of the transit of Venus party to China, it was dropped, never to be resumed. The charge of this expedition was Watson's most important scientific commission. In a letter written to Benjamin Peirce, in 1873, he had, in opposition to LeVerrier's well-known views, expressed the opinion that the transit of Venus should be observed by astronomers as extensively as possible, and that every device known to science should be brought to bear upon these observations for the determination of the solar parallax. It is interesting now to note that in this letter he expresses the opinion that observations of contacts and measurements with the heliometer will be found to give a much more trustworthy determination of the parallax than can be obtained by the photographic method, but he joins to this the statement that the photographic method ought to be thoroughly tested.

Watson undertook the conduct of the expedition to China deeply impressed with the responsibility which it imposed upon him, and the last six months of 1874 were among the most laborious and oppressive of his life. It was, therefore, with a sense of profound relief that he saw this period of arduous labor crowned with the success of his party in the observation of the transit at Peking. It is interesting to note that on October 10, 1874, at Peking, he discovered the minor planet Juwra (139). The return from China was made leisurely *via* India, Egypt, and Europe, several weeks being spent in Egypt, at the invitation of the Khedive, in instructing and co-operating with the engineer officers of the Egyptian

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army in the first steps toward a geodetic survey of that country. This work, performed by Watson without pecuniary compensation, won for him the cordial thanks of the Khedive and the decoration of Knight Commander of the Imperial Order of the Medjidich of Turkey and Egypt.

Close following upon Watson's return to America came his appointment as one of the judges at the International Centennial Exposition of 1876, in connection with which he prepared an elaborate report upon the horological instruments there exhibited.

The year 1878 brought a new subject, which engaged much of Watson's attention during the few remaining years of his life. He had corresponded with LeVerrier about the supposed planet Vulcan, and believed firmly in its existence, and, at LeVerrier's request, had co-operated with him in securing observations of the sun's disk at the times of expected transits of the planet. The eclipse of 1878 offered a favorable opportunity of search for this body, of which he eagerly availed himself, and mounted his telescope upon the crest of the Rocky Mountains at Separation, in Wyoming Territory. We need not here recount the details of the observations which led to the announcement of the discovery of two new bodies supposed to be intra-mercurial planets, since Watson has himself given an account of these in the *American Journal of Science* and in the *Astronomische Nachrichten*. Suffice it to say that he returned home firm in the conviction that he had discovered the unknown Vulcan and, perhaps, another planet as well. Uncertainty as to the latter object soon gave way to confidence that both the bodies seen by him must be major planets moving within the orbit of Mercury. The scientific world was skeptical, but he would convince it that its lack of faith was unwarranted.

The directorship of the new observatory founded at Madison, Wisconsin, by the liberality of ex-Governor Washburn had before this been offered him, but he had hesitated, unwilling to leave the surroundings in which the greater part of his life had been passed. But the scale was now turned, and the promised superior equipment of the new observatory carried him to a new home, confident of speedily demonstrating the reality of his discoveries.

He removed to Madison and entered upon the directorship of the Washburn Observatory in the spring of 1879. The observatory was then far from complete; its large equatorial was mounted, but was its only instrument, and Watson's energies were spent, even to

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the last hours of his life, in designing and superintending the construction of new buildings and new apparatus. In the midst of this activity time was found for a return to the problem of telescope-building, which had occupied his student days. Optical glass was procured and plans laid for the construction, under his personal supervision, of several objectives which were to embody his own ideas upon this art, and work was actually commenced upon one large reflecting telescope. But no stress of other work or other interests could displace Vulcan from his mind. A scheme was devised whereby he should be able to observe the planet at noon-day without the intervention of an eclipse. The hill upon which the observatory stands slopes sharply to the south. At the foot of this hill was dug a deep cellar with a tube extending from it through the soil, parallel to the earth's axis and terminating in a masonry pier at the top of the hill. A telescope was to be so mounted in the cellar as to point up through the tube to a heliostat mounted at its upper end, by which rays of light coming from the sun or other celestial body might be directed into the telescope. The tube, fifty-six feet in length, was to serve as a long dew-cap and enable the observer to sweep close up to the sun's limb without being blinded by the stray light surrounding it. So confident was Watson of the success of this device, and that by its aid Vulcan could be refound, that he did not hesitate to undertake its construction at an expense to himself of several thousand dollars.

He did not live to see its completion, nor the fruition of any of the plans which he had formed for the observatory. Signs, more easily recognized after his death than at the time of their occurrence, pointed to a diminishing vitality and to a weakening of his physical powers. There was, however, no diminution of an activity which exposed him daily to the inclemencies of an approaching winter, and his rotund figure and ruddy face seemed to give little cause for apprehension. Stricken down by a congestive chill, from which he partially rallied and then relapsed, he died, November 22, 1880, within forty-eight hours after the suspension of his ordinary daily work.

Watson's scientific work was far from being the measure of his life, and any estimate of his career which did not take into account other sides of his character would be far indeed from the mark. The stern experiences of his boyhood had stunted the growth of qualities which a more genial lot might have developed in him, and

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in estimating his character as a man this early training must not be overlooked. It had taught him the value of money, and he eagerly sought its acquisition as a source of power. He engaged in business enterprises, and became an insurance agent, a photographer, a bookseller, a printer and publisher, and an insurance actuary, with moderate pecuniary success, but with the result of acquiring a peculiar power and influence over men of affairs, which he used to the marked advantage of the educational and scientific institutions with which he was connected.

The subject of life insurance early attracted his attention and interest, and for nearly half his life he was engaged in its practical workings. I cannot do better than quote in this connection the words of one of his colleagues in the University of Michigan, Hon. Thomas M. Cooley, long upon the supreme bench of that state. "Few knew so well as I did the valuable services he rendered to the people in placing the great interest of life insurance upon a solid foundation. He understood thoroughly the principles of this business and was impatient that irresponsible organizations, by deception and fraudulent contrivances, should draw money from the people under the false pretense of insuring their families against loss by death. He thought, too, that there should be home organizations, whereby the vast and steady flow of money from the State should be stopped, and the accruing profits from insurance retained and expended among our own people. When at last such an organization was perfected he was invited, quite unexpectedly to himself, to be its actuary, and so invaluable have his services been found that his judgment has come to be accepted as law by the able business men who have been at the head of its affairs. * * * Some of us had personal knowledge that more than one state legislature invited his assistance in framing insurance laws, and that he had large influence in preventing crude and mischievous legislation on a subject with which the general public is unfamiliar, and concerning which those who think they know it well are generally most profoundly ignorant."

There lies before the writer of this sketch a letter from the secretary of the company whose actuary Watson was, confirming this estimate of his services and attributing the success of the company largely to his influence. Watson's long connection with insurance matters led in his later years to the preparation and publication of

extensive tables—his Interest and Investment Tables—for facilitating commercial and financial computations.

As a college professor Watson had great popularity among his students, but it was not a popularity of the best kind. His classroom work was conducted upon the avowed theory that his duty was to help those who sought a knowledge of astronomy and not to coerce into study the indifferent and careless. The latter class flocked in great numbers to his lectures and were delighted with the fluency and easy grace which imparted a charm to his discourse, and which carried them over the allotted ground with little expenditure of time or thought. Watson's instruction was mainly given by lectures, and the felicity of his discourse cannot be better illustrated than by a scene which will not readily pass from the memory of those who witnessed it. He had been summoned as an expert witness in an action to recover the insurance upon a building destroyed by a tornado, and was to testify in regard to the laws of storms. The examination began in the usual dry and formal manner, by direct question and answer, but the questions grew fewer and the answers longer until, their surroundings forgotten, judge and jury, attorneys and spectators, sat listening to a popular exposition of the science of meteorology, going far outside the scope of the case at bar.

Watson was singularly indifferent to the opinions of the community in which he lived—an indifference rendered the more remarkable by the fact that he took a lively interest in local affairs and local controversies. He had bitter enemies, and they circulated reports, to the discredit of his personal character, which went uncontradicted and gained an undeserved credence. It cannot be denied that a measure of truth attended many of these statements, but they were habitually distorted and magnified out of all proportion. He wished his life and character to be estimated by the world at large. His scientific reputation he valued more highly than local esteem. "Let that be established," he was wont to say, "and opinion here will fall in with it"—a view partly, but not entirely, justified by the event. Within the circle of his own family he was a generous man, and in his college relations his pre-eminent abilities were freely placed at the service of such of his colleagues as needed them. For the ordinary forms of social intercourse he had no taste, and held himself aloof from them, giving to his work hours that others spent in recreation, thus crowding more of achievement into

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the years of his life, but, in the judgment of his friends, lessening their number.

Watson was elected a member of the National Academy of Sciences in 1868, and by his will the National Academy was made the residuary legatee of his estate, which is "to be aggregated, kept, and invested as a perpetual fund, the income from which shall be expended by said academy for the promotion of astronomical science." The academy has accepted this trust, and administers it in accordance with the following provisions of the will:

"In order to carry out the wish hereinbefore expressed as to the disposal of the income from the fund resulting from my estate hereby devised to said National Academy of Sciences, I do hereby direct that the designation of the particular objects and works which may be aided by this fund shall be determined, subject to approval by a vote of the academy, by a board of trustees, three in number, who shall be members of the academy, and elected after the first herein named by said academy whenever a vacancy may occur, by death or otherwise. The trustees so appointed shall hold said office unless voluntarily relinquished by them during the period of their membership in the said National Academy of Sciences, and I do hereby appoint and constitute *Julius E. Hilgard*, of the United States Coast Survey, and *Simon Newcomb* and *J. H. C. Coffin*, professors of mathematics, U. S. Navy, all of Washington, in the District of Columbia, to be the first board of trustees for the purposes herein named.

"It is my wish that the academy may, if it shall seem proper, provide for a gold medal of the value of one hundred dollars, to be awarded, with a further gratuity of one hundred dollars, from time to time, to the person in any country who shall make any astronomical discovery or produce any astronomical work worthy of special reward as contributing to our science. It is my further wish that provision be made for preparing and publishing tables of the motion of all the planets which have been discovered by me as soon as it may be practicable to do so; and I desire that in all cases the trustees and the academy shall act in harmony to obtain results of greatest possible aid to our science from the income fund resulting from my estate. I desire that results so obtained shall be published as speedily as possible, in such manner as may be provided by the academy."

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The amount of the fund, principal and accrued interest, was, in April, 1886, a little less than \$15,000. The medal provided by the will has been awarded but once. At the April, 1886, meeting of the National Academy it was, at the recommendation of the board of trustees of the Watson fund, "Resolved, That the Watson medal and the further sum of \$100 in gold be awarded to Dr. Benjamin Apthorp Gould, for his valuable labors for nearly forty years in promoting the progress in astronomical science, and especially for his successful establishment of the National Observatory of the Argentine Republic, as manifested in the six volumes of observations recently prepared and published by him.

I close this brief sketch of Watson's life with a list of his more important published works, omitting the numerous observations, ephemerides, and notices of the discovery of minor planets which are contained in the astronomical periodicals. In lieu of the latter there is appended a list of the twenty-two planets discovered by him, together with the dates of their discovery.

A List of the More Important Published Writings of James C. Watson.

- On the Extraction of Roots: *Michigan School Journal* (1859).
- On the Orbit of Pandora (55): *Brünnow's Astron. Not.*, Vol. 1, p. 59.
- On the Orbit of Hestia (46): *Brünnow's Astron. Not.*, Vol. 1, p. 121.
- A Popular Treatise on Comets: *Philadelphia*, 1861.
- Correction of the Elements of the Orbit of a Comet: *Am. Jour. Sci.*, 1863, p. 218.
- Investigation of the Orbit of Eurynome (79): *Astr. Nachr.*, Vol. 64, col. 23.
- Theoretical Astronomy relating to the Motions of the Heavenly Bodies around the Sun in accordance with the law of Universal Gravitation: *Philadelphia*, 1868.
- Horological Instruments. United States International Exhibition, 1876. Reports and Awards. Group XXV, p. 56: *Washington*, 1880.
- Discovery of an Intra-Mercurial Planet: *Astr. Nachr.*, Vol. 93, col. 141, 161, 189, 239; Vol. 95, 102. *Am. Jour. Sci.*, Vol. 16, pp. 230, 310.
- Watson's Interest and Investment Tables: *Ann Arbor*, 1879.

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List of Minor Planets Discovered by James C. Watson.

No.	Name.	Date of discovery.	No.	Name.	Date of discovery.
79	Eurynome	1863, Sept. 14	121	Hermione	1872, May 12
93	Minerva	1867, Aug. 24	128	Nemesis	" Nov. 25
94	Aurora	1867, Sept. 6	132	Aethra	1873, June 13
100	Hekate	1868, July 11	133	Cyrene	" Aug. 16
101	Helena	" Aug. 15	139	Juwa	1874, Oct. 10
103	Hera	" Sept. 7	150	Nuwa	1875, Oct. 18
104	Klymene	" Sept. 13	161	Athor	1876, Apr. 16
105	Artemis	" Sept. 16	168	Sibylla	" Sept. 28
106	Dione	" Oct. 10	174	Phaedra	1877, Sept. 2
115	Thyra	1871, Aug. 6	175	Andromache	" Oct. 1
119	Althaea	1872, Apr. 8	179	Klytaemnestra	" Nov. 11

MEMOIR
OF
JAMES BUCHANAN EADS.
1820-1887.

BY
WILLIAM SELLERS.

READ BEFORE THE NATIONAL ACADEMY APRIL, 1888.

MEMOIR OF JAMES BUCHANAN EADS.

JAMES B. EADS was born at Lawrenceburgh, Indiana, May 23rd, 1820, at which place his father, Thomas C. Eads, was engaged in mercantile business. A few years after young Eads' birth his father, having met with reverses in Lawrenceburgh, removed with his family to Louisville, Kentucky, and soon afterwards to St. Louis. At this time the family consisted of Thomas C. Eads, his wife, son, and two daughters. The steamboat on which they took passage to St. Louis was burned, their household goods were all destroyed, and young Eads, then about 13 years of age, found himself in St. Louis under the immediate necessity of finding something to do to assist in the support of the family, and this first employment was peddling apples from a basket. In this capacity he doubtless attracted the attention of Mr. Williams, of the mercantile house of Williams and Durings, for with this firm he was soon employed as a clerk. The senior member, impressed with his ability, invited him to use his library, and the lad, who had been obliged to leave school at twelve years of age, was thus enabled to resume his studies to such purpose that, although he never after attended school in later life, his varied acquirements were recognized as large, even among men wholly devoted to science or letters.

His next employment was that of purser on a steamboat on the Mississippi river, in which position he not only had the opportunity to use the mercantile knowledge that he had previously acquired, but also to recognize the urgent necessity for saving the wrecks of boats abandoned along the river. Other men had long before the same opportunity, but Eads alone perceived its importance. He first devised a diving bell for recovering cargoes, and, succeeding in this, he fitted out a large vessel with appliances for lifting the entire hull with its cargo, and formed a company for carrying on wrecking operations over the whole river and its tributaries. He was very successful in this business, but it required him to be moving about nearly all of his time. He was engaged to be married and desired a home, so that in 1845 he sold out his interest and engaged in the manufacture of glass in St. Louis. Within three years this proved to be a disastrous failure, in which was swept away all of his

fortune, and worse than all, it left him \$25,000 in debt, with a young family dependent upon him. He now resumed his former business upon a capital of \$1,500, borrowed from his creditors. In this he was again eminently successful, so that soon he had paid off all his creditors in full, and in 1855-'6 he made his first proposition to the General Government to contract to remove from the Mississippi river snags and other obstructions to navigation and to keep the channel free and open for a term of years. The bill passed the House of Representatives, but the Senate failed to take action. The proposition itself is evidence of Mr. Eads' bold and enterprising spirit and far-seeing sagacity. In the business in which he was now engaged his opportunities for studying the river were great and were not lost upon him. The laws which governed its flow and determined its deposits were studied with such care and precision that when later the opportunity came for their application he was found fully equipped for the solution of the problem. His success in the management of a large enterprise had given him reputation at home, which was soon to be greatly extended, for almost immediately after the surrender of Fort Sumter he was summoned to Washington to give his views upon the best methods of utilizing our Western waters for attack and defense; these he gave in a written statement, advising the use of gun-boats propelled by steam and of peculiar construction, the casemates slanting back at an angle of 35 degrees from the water line, armor-plated, and extending entirely around the vessel, forming a rectangular gun deck. The Quartermaster General advertised for bids and Mr. Eads' proposal was accepted. The contract was executed August 7th, 1861, by which Mr. Eads bound himself to construct seven of these vessels, having a capacity of about 600 tons each, and to be ready for armament in sixty-five days. None but a bold and self-reliant man would have ventured to make such a contract. The manufactories of the country were paralyzed, workmen were scattered and enlisting in the Army, and it seemed hopeless to expect the completion of the work even approximately to the time specified. The result is graphically described by Boynton in his "History of the Navy during the Rebellion," Vol. 1, pp. 502-504:

"The signatures were scarcely dry upon this important contract before persons in different parts of the country were employed upon the work through telegraphic orders issued from Washington. Special agents were dispatched in every direction, and saw-mills

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were simultaneously occupied in cutting the timber required in the construction of the vessels in Kentucky, Tennessee, Illinois, Indiana, Ohio, Minnesota, and Missouri, and railroads, steamboats, and barges were engaged for its immediate transportation. Nearly all of the largest machine shops and foundries in St. Louis, and many small ones, were at once set at work day and night, and the telegraph lines between St. Louis and Pittsburgh and Cincinnati were occupied frequently for hours in transmitting instructions to similar establishments in those cities for the construction of the twenty-one steam-engines and the five and thirty steam-boilers that were to propel the fleet. The large rolling mills of Gaylord, Sons & Co., at Portsmouth, Ohio; those of Swift & Co., at Newport, Ky.; and of Harrison, Chouteau & Valle, at St. Louis, were all employed in rolling armor plate. Mr. Thomas G. Gaylord, of Cincinnati, undertook to furnish this important material, and his promptness and energy greatly contributed to the rapid progress of the work. To him justly belongs the credit of rolling the first iron plating used in the war. Within two weeks not less than four thousand men were engaged in the various details of its construction. Neither the sanctity of the Sabbath nor the darkness of night were permitted to interrupt it. The workmen on the hulls were promised a handsome bonus in money for each one who stood steadfastly at the work until it was completed, and many thousands of dollars were thus gratuitously paid by Mr. Eads when it was finished. On the 12th of October, 1861, the first United States iron-clad, with her boilers and engines on board, was launched in Carondelet, Mo., in forty-five days from the laying of her keel. She was named the *St. Louis* by Rear Admiral Foote, in honor of the city. In ten days after the *Carondelet* was launched, and the *Cincinnati*, *Louisville*, *Mound City*, *Cairo*, and *Pittsburgh* followed in rapid succession. An eighth vessel, larger, more powerful, and superior in every respect, was also undertaken before the hulls of the first seven had fairly assumed shape. Thus one individual put in construction and pushed to completion within one hundred days a powerful squadron of eight steamers, aggregating five thousand tons, capable of steaming at nine knots per hour, each heavily armored, fully equipped, and all ready for their armament of one hundred and seven large guns. The fact that such a work was done is nobler praise than any that can be bestowed by words."

Mr. Eads constructed in all fourteen heavily armored gun-boats

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and converted seven transports into what were called "tin-clads" or musket-proof gun-boats, and he built in addition four heavy mortar boats.

His professional career cannot be more fittingly described than by the following from the proceedings of the Am. Soc. of C. E. :

"His next work was the construction of the arched bridge over the Mississippi river at St. Louis, with whose history and plans most of the older members of the Society are familiar; hence only its principal features will be described. Since its construction, in the years 1867-'74, many important bridges have been built and deep foundations sunken, but at the time the work was commenced on this bridge there were no well-known precedents for some of its principal features or dimensions. The sudden changes in the river bed at this point made it necessary to go to the bed-rock for the foundation of the piers. The base of one pier is 136 feet below high water, and it was sunken through 90 feet of sand and gravel; another is 130 feet below high water, and it went through 80 feet of sand.

"The piers are massive structures, one of them weighing 45,000 tons. The central span is 520 feet in the clear, and the two side spans 502 feet. The plan of the superstructure is a ribbed arch carrying a double track railway with a broad wagon-way above.

"By the requirements of the act of Congress the clear height was fixed at 50 feet above the city directrix.

"To show how doubtful was considered the practicability of erecting spans of the length required the following extract is given from the resolutions adopted by a convention composed of twenty-eight of the leading civil engineers of the United States, and whose names were appended to the report of the convention :

"*Resolved*, That we as practical engineers cannot conscientiously recommend to the parties in interest to venture upon the construction of spans of as great length as the maximum one prescribed by law' (500 feet).

"Mr. Eads, however, so clearly proved the correctness of his plans and showed that engineers of international reputation had designed even longer spans, which had been approved by their brother engineers, and that there were in existence at the time bridges whose spans were nearly equal to his proposed plans, that the Bridge Company were fully satisfied with his designs, and he proceeded with the construction of the work.

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"The years spent in this great enterprise were fraught with care, anxieties, and constant labor.

"No one can read his masterly reports upon this bridge without being deeply impressed, not only with his intimate knowledge of the details of the work, but with the fact that he designed nearly all of them himself. The conditions were so extraordinary in many respects that it required a master mind, a real mechanical genius, to devise and invent many appliances, particularly for the deep foundation work. The 'Eads sand pump,' a model of which is in the possession of the society, although very simple, was a novel device, and utilized forces in such a way as to greatly facilitate the removal of the material from under the caissons. Many of the peculiar arrangements of the air locks were devised by Mr. Eads for that bridge, and some of them were afterwards used in sinking the piers of the Brooklyn bridge.

"In 1881 the historian of the St. Louis bridge, Professor C. M. Woodward, Assoc. Am. Soc. C. E., of Washington University, paid the following well-deserved tribute to the engineering of the east abutment of the bridge: 'The construction of the east abutment was a signal triumph of engineering. It was quite unparalleled, both in size and in the depth to which it was sunk, and it stands to-day the deepest subaqueous foundation ever built.'

"As to the superstructure, it marked an era in bridge-building, and particularly in the character of materials. The obstacles met with in the manufacture of steel to meet the requirements of spans of this kind and length were serious and many. The steel-makers found that their facilities were inadequate to the magnitude of the work they had undertaken; they had no workmen of sufficient skill, and their foreman had never worked steel in such large masses. The rigid tests, both for maximum strength and for elastic limit, were new, and the character of the workmanship was quite exceptional.

"Speaking himself of the bridge, and of the various unforeseen expenses and difficulties that had attended its construction, Mr. Eads, in his report to the Bridge Company in 1871, closes as follows:

"'When all of the many difficulties that have retarded this great work shall have at last been surmounted and the bridge becomes an accomplished fact, it will be found unequalled in the important qualities of strength, durability, capacity, and magnitude by any

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similar structure in the world. Its great usefulness, undoubted safety, and beautiful proportions will constitute it a national bridge, entitling those through whose individual wealth it has been created to the respect of their fellow-men, while its imperishable construction will convey to future ages a noble record of the enterprise and intelligence which mark the present times.'

"The magnitude of the work may be appreciated from the following summary of its dimensions, quantities, and weights:

"Length of the piers at base, about 85 feet; width, about 60 feet; length at top, about 63 feet; width, about 24 feet.

"Height from bed-rock to top of masonry: greatest, 192 feet 9 inches; least, 112 feet 8½ inches.

"Foundations below low water: greatest depth, 94 feet; least depth, 13 feet; below high water: greatest, 135 feet 7 inches; least, 54 feet 7 inches.

"Total amount of masonry and concrete, 102,897.6 cubic yards.

"Total weight of steel, iron, wood, and tracks, 13,135,500 pounds.

"The following, from London 'Engineering,' written just before the completion of the bridge, will show the appreciation by the engineering profession in England of the magnitude of the work and of the great qualities demanded for its success:

"Our present requirement being to select some example of the most highly developed type of bridge-building of the present day, we have no difficulty in passing before ourselves in mental review the different works now in progress throughout the world, and we have still less difficulty in selecting as our example the magnificent arched bridge now almost completed by Captain Eads at St. Louis. In that work the alliance between the theorist and the practical man is complete. The highest powers of modern analysis have been called into requisition for the determination of the strains; the resources of the manufacturer have been taxed to the utmost in production of material and perfection of workmanship, and the ingenuity of the builder has been alike taxed to put the unprecedented mass into place. In short, brain power has been called into action in every department. One long-sighed-for result, the welding of the theoretical and practical man into one homogeneous mass, without which no truly great undertaking could possibly be carried out, has at last been attained.'

"The St. Louis bridge was scarcely completed when Mr. Eads turned his attention practically to a subject that had long been in

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his mind and desires, since his proposition to the Government in 1856 to open up and maintain the channels of the rivers, and that was the opening of the mouth of the Mississippi river, where the sand bars lying at the embouchure of the passes into the Gulf had become a very serious obstruction to the commerce between the Mississippi Valley and the ocean.

"In February, 1874, he made a formal proposition to Congress to open the mouth of the Southwest Pass and maintain the channel. This he agreed to do at the sole risk of himself and associates. The attacks upon his proposition from all sides and the gallant and victorious fight which he waged single-handed, in Congress and out of it, have already become a part of the history of this important work and need not be repeated here. The valor, fortitude, and persistence of Mr. Eads in these controversies only showed his natural traits of character and revealed an adversary in polemic discussion that few cared to meet. His papers, addresses, and communications to Congress and to the magazines and newspapers during the preliminary stages and the construction of this work are probably unsurpassed in value as engineering expositions of the principles controlling the flow of water, the movement and deposition of sediment, and of the correct methods of river improvements. The principal opposition to his proposal came from those army engineers who had proposed to solve the problem by building a canal from Fort St. Philip, on the main river above the head of the passes, to the adjacent sound. After considerable discussion a commission of engineers, composed of three army engineers, three civil engineers, and a member of the United States Coast Survey, was authorized by Congress.

"At the next session of Congress this commission, after personally examining certain important rivers of Europe which had been improved by jetties, reported in favor of the plan proposed by Mr. Eads, and recommended its application to the South Pass, the smallest of the three, where the depth on the bar was about eight feet, in place of the Southwest Pass, selected by Mr. Eads, where the depth was over twelve feet.

"The work was commenced in the summer of 1875, the construction of which required about four years, the channel demanded by the contract with the Government having been obtained in July, 1879. Its dimensions were a depth of 26 feet, with a width of 200

feet at that depth, and a central depth of 30 feet, without regard to width.

"Mr. Eads brought to the construction of this important work the same genius that had characterized his management of the St. Louis bridge. No obstacle, whether of an engineering or financial character, dismayed or even discouraged him. His great qualities were exhibited on this work in a marked degree. His knowledge of the laws of currents, his predictions of complete success by working in accordance with these laws, his unalterable determination to achieve success, and his unfaltering faith in the darkest hours of that work were indelibly impressed upon the minds of his intimate associates, so that, whatever his detractors said, those who knew him best felt the inspiration of his great genius, a confidence in his consummate skill, and a repose in his unswerving confidence in the final result.

"The following words from an address delivered at St. Louis immediately after the passage of the jetty act show the high purpose and the implicit faith of Mr. Eads. He rested calmly in the operation of laws which he knew from practical experience and careful study were controlling the forces with which he was dealing:

"If the profession of an engineer were not based upon exact science I might tremble for the result, in view of the immensity of the interests which are dependent upon my success. But every atom that moves onward in the river, from the moment it leaves its home amid crystal springs or mountain snows, throughout the 1,500 leagues of its devious pathway, until it is finally lost in the vast waters of the Gulf, is controlled by laws as fixed and certain as those which direct the majestic march of the heavenly spheres. Every phenomenon and apparent eccentricity of the river, its scouring and depositing action, its caving banks, the formation of the bars at its mouth, the effect of the waves and tides of the sea upon its currents and deposits, are controlled by laws as immutable as the Creator, and the engineer needs only to be assured that he does not ignore the existence of any of these laws to feel positively certain of the result he aims at.

"I therefore undertake the work with a faith based upon the ever-constant ordinance of God himself; and so certain as He will spare my life and faculties for two years more I will give to the Mississippi river, through His grace and by the application of His laws, a deep, open, safe, and permanent outlet to the sea.'

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"The jetties which Mr. Eads designed and built extend from the land's end at the mouth of South Pass about two and a quarter miles out over the bar and into deep water in the Gulf. The jetties are parallel, and the channel between them is about seven hundred feet wide. They are built of willow mattresses sunken by stone and capped near the sea ends with massive concrete blocks, the largest of which weigh 265 tons, and were, at the time they were put in position, the largest blocks ever placed on sea walls.

"The works at the head of the passes required skillful designing, for the conditions there were very complicated. By employing permeable constructions deposits were induced and the river compelled to excavate, by its accelerated current, a channel through the bar which had obstructed the entrance to South Pass and on which there were but 15 feet of water.

"As at the jetties, so at the head of the passes, the works have simply assisted nature in opening channels and in building banks.

"When we contemplate the fact that these works are composed almost wholly of light willows, with a large portion of the mattresses standing on edge, simply as screens to check the current and cause deposit, they constitute a remarkable illustration of how completely the immense forces of nature may sometimes be controlled by a wise use of the most inexpensive and unsubstantial materials, which nature seemingly places within convenient reach of man for the very purpose.

"Here by the gentlest influences the mighty current is swayed and directed completely obedient to his will. There is no instance indeed in the world where such a vast volume of water is placed under such absolute and permanent control of the engineer through methods so economic and simple as those adopted at the head of the passes of the Mississippi river.

"The character of this work, its great importance to the commercial interests of the country and the world, its successful accomplishment, the continued maintenance of the deep and wide channel which the work created, the difficult engineering problems which were solved so successfully, all conspired to place Mr. Eads in the foremost rank of hydraulic engineers, as his great bridge had already placed him in the front rank of bridge engineers.

"The unselfish patriotism and desire to promote the welfare of his country are seen in nothing in his life more than in his persistent efforts to secure the improvement of the Mississippi river from the

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Gulf to the mouth of the Ohio. Some of the most laborious years of his life were spent in efforts to obtain the legislation from Congress necessary to inaugurate a comprehensive system of river improvement under a mixed commission. It is not enough to say that he did more than any one to accomplish this: it is only the truth to say that without his untiring efforts neither the commission nor the improvement works would have existed.

"In the clearest and most convincing manner he stated the plan of improvement in documents addressed to Congress and in addresses before public meetings in the Mississippi Valley. Between 1874 and 1879 he outlined one of the most magnificent and comprehensive plans which hydraulic engineers ever undertook in this or any other country. By untiring efforts he at last brought about a public sentiment in favor of the improvement and obtained the necessary Congressional action.

"The commission was appointed in 1879 and presented its preliminary report February 17th, 1880. He was appointed on this commission and served two years, when his failing health and the cares of other important business compelled him to resign and go abroad. He, however, to the time of his death, was most deeply interested in this great work, and deplored what he believed to be the departure of the commission from the true principles of the improvement, and combatted false methods with all the strength of his great intellect. This is no place to indulge in controversial discussions, but it is only a duty to Mr. Eads to make a brief statement of the views and plans for whose adoption, execution, and preservation he had been for several years such an earnest and untiring advocate.

"The improvement of the great river and the welfare of the great valley were among the most cherished objects of his life, and he could not brook any departure from the correct principles, even though it was counseled by his best friends. As far as this important question was concerned, they were his enemies who opposed its correct treatment, and he mercilessly fought them in order that he might bring about the adoption of correct plans in place of what he believed to be incorrect ones. This intense earnestness in the discussion of this, to him, vital question often made him appear a bitter and unrelenting foe to his opponents, but no man ever lived with a kinder heart than he, even towards those who opposed him. To him the unfolding of great and correct principles was more than

personal friendships. His beliefs were his friends if they held within their grasp and scope the weal of humanity.

"The following extracts are from his 'Minority Report of Mississippi River Commission, April, 1882:'

"I have named three controlling principles which are present in every problem presented by the characteristic phenomena of the river. Each one of these is very simple in itself. It is, however, absolutely necessary to remember each of them to fully comprehend the subject and to be able to recognize the respective influence of each in creating these phenomena. I will briefly repeat them to more strongly impress their importance: The first is the force producing the current. This force is simply the result of the fall of the river from a higher to a lower level. The second is the frictional resistance of its bed. The third is the intimate relation between the quantity of sediment carried in the water and the velocity of the current. If we increase or decrease the current from any cause we increase or decrease the quantity of sediment carried by the river. We can increase or decrease the current temporarily by either of two methods, namely, by altering the slope or by altering the frictional resistance. Therefore by these two methods the scouring and depositing effect can be produced. If we increase the current during the floods we produce a greater deepening and enlarging of the channel through the shoals, and they are left in better condition during low water, and at the same time we ultimately lower the height of the flood. If we decrease the current we produce shoals and higher floods.'

"He constantly and persistently held that the 'jetty' principle, which the Commission originally adopted, should not be departed from; that the contraction of the channel by permeable works to its normal width of about 3,000 feet would accomplish the desired result, which would be not only a deepening of the channel for navigation, but a restoration of the bed of the river to an approximately uniform plane in place of the strikingly irregular bed, with its deep holes and uplifting bars, which were due entirely to the varying widths of the river in its unrectified condition, and that therefore it was utterly useless to waste money in impracticable attempts to hold the caving concave banks of the river by costly revetments, which, he contended, had already swallowed up millions of public money without any useful result to show for it.

"From the first he contended that the proper improvement of

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the river by deepening the channel would decrease the frictional resistance to the current by lessening the surface exposed to it ; that then the flood volume would flow on lower slopes ; that the practical result would be the lowering of these floods ; that the necessity for high levees would no longer exist, and that the 30,000 square miles of rich alluvial lands, now subject to inundation, would be effectually reclaimed by the same works that produced and maintained a deep channel from Cairo to New Orleans.

"In all the years during which he worked and fought for this grand result he had no selfish interest to subserve, no contract to execute, nothing himself to gain, and he had no thought even of the fame that would come to him as a crown of his unremitting efforts, for wealth he had already, and fame was not wanting.

"The channel at the mouth of the Mississippi river was obtained in July, 1879. The Panama Canal Congress was held in May of the same year.

"The attention of the civilized world was directed as never before to an interoceanic transit-way. Mr. Eads conceived the idea of extending the Mississippi river, commercially speaking, into the Pacific Ocean, and of opening up to the eastern coast of Mexico and the States bordering on the Gulf and to the great valley of the Mississippi the rich markets of the Pacific, and at the same time to connect the Atlantic and Pacific coasts of the United States by the shortest possible route by way of the Tehuantepec Isthmus, where a crossing for ships would effect a saving of 2,000 miles over the Panama route and 1,500 miles over the Nicaragua route. As a canal was impracticable at Tehuantepec he proposed to build a ship railway for the transportation of ocean vessels over the 140 miles of land that separate the Gulf of Mexico from the Pacific Ocean. He at once began the preliminary plans for the work and made a careful study of the subject in the fall and winter of 1879.

"On March 9th and 13th, 1880, he appeared before the Select Committee of the House of Representatives on Interoceanic Canals and replied to Count De Lesseps, who was advocating the construction of the Panama Canal. In his remarks Mr. Eads explained in considerable detail his plans for a ship railway and contended that it was entirely practicable. On August 11th, 1880, he delivered an address before the San Francisco Chamber of Commerce on the 'Interoceanic Ship Railway,' in which he said :

" ' Standing in your presence to-day and conscious of the full im-

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port of my words, I declare to you, *first*, that a ship railway can be constructed at one-half the cost of a canal with locks, and in one-half the time; *second*, that when completed the railway can be maintained and operated at a cost not exceeding that of a canal; *third*, that your largest vessels with their cargoes can be safely carried from ocean to ocean in one-half the time required for a passage through the canal.' And he then gave additional reasons for his preference for a railway.

"In November, 1880, he went to Mexico and obtained a valuable concession from the Mexican Government for building a ship railway. He instituted the preliminary surveys by the assistance of that Government and went with the engineering party to the Isthmus on board a Mexican Government vessel which had been placed at his disposal.

"In March, 1881, he presented his views on this subject in the North American Review and explained and enforced the controlling principles at considerable length

"In the winter of 1881 he made a proposition to Congress to build the railway at his own expense and at his own risk, provided the Government would guarantee a dividend of 6 per cent. for fifteen years after he had, by the actual construction and operation of the railway, proven its practicability. His views of the feasibility of the railway were supported by the professional opinions of a large number of practical experts both in the United States and England, and the Senate and House committees favorably reported the bill, but the Senate failed to take action upon it.

"In 1885 Mr. Eads obtained a modification of the concession from Mexico by which that Government guaranteed that one-third of the net revenue should amount to one million two hundred and fifty thousand dollars (\$1,250,000) per annum, and granted several other important changes which increased the value of the concession.

"He then introduced a new bill in Congress, by which, when the ship railway should be entirely completed and put into operation, transporting large ocean vessels, fully laden, the Government guaranteed that two-thirds of the net revenue should amount to two million five hundred thousand dollars (\$2,500,000), Mexico having guaranteed the other third.

"This bill was favorably reported by the committee of the two Houses before the end of that session, but in the last session of Congress it was deemed advisable to exclude all guarantee clauses and

ask for a simple charter. Mr. Eads went to Washington this last winter—in January—although in very poor health, to secure the passage of the act, confidently believing that it would insure the raising of the capital, in this country and England, necessary to build the railway. He was not able to remain in Washington, and, by the advice of his physician and friends, sailed for Nassau, N. P., Bahamas, where he died after a short illness. After his arrival at Nassau his health rapidly improved, but he contracted a slight cold, which resulted in congestion of the lungs. This was the remote cause of his death, but the direct cause was uræmia, from which he had been a great sufferer for several years.

“During these years and until his death Mr. Eads not only advocated the important project of a ship railway, but devoted much of his time and attention to the preliminary work necessary to place the enterprise squarely and fully before the world. He did not hesitate to support his opinions with his money, and although he had associated with himself about ninety promoters, all financially interested in the work, he went far beyond any of them in furnishing the necessary funds for making elaborate surveys, plans, and a model illustrating them and in meeting the expenses incident to so great a work. His views, advanced by himself and those associated with him and sent to all parts of the civilized world, have gained a firm foothold in the belief of thinking men everywhere.

“His last engineering work, while sick at Nassau, was the examination and review of the plans for an improved hydraulic lift for the ship railway, which he had placed in the hands of a mechanical engineer before he left this country.

“He exhibited in his last hours the same heroic courage and sublime faith that characterized his entire life. He was not afraid to die, but such was his devotion to the great enterprise in which he was engaged, so strong was his presentiment that he would live to see the fruition of his hopes, that he could not think of leaving the world until his work was done and he should see with his own eyes what the great poet saw in vision:

“ ‘Lo, ships from seas by nature barred
Mount along ways by man prepared,
Along far-stretching vales, whose streams
Seek other seas; their canvas gleams
And busy towns grow up on coasts
Thronged yesterday by airy ghosts.’

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"The pure motives which actuated Mr. Eads in this great project and the lofty purpose that led him to devote his life and bend all his energies to its accomplishment can be clearly read between the lines of the following extract from his address before the Boston Society of Arts May 19th, 1885:

"When this work is completed, as I am sure it will be, and that long before a canal is cut across the American isthmus, the benefits will be felt by our fellow-men all over the world, not only in lessening the cost of transportation on the necessities and luxuries of life and in shortening the long, weary, trackless distances which now separate nations from each other and by opening new markets for the multitude of commodities which are interchanged by the various peoples of the earth, but also by bringing distant communities into more social and commercial relation with each other, whereby the better sympathies and sentiments of each will be promoted, their prejudices removed, the amenities of life increased, and the benefits of civilization, science, and religion more surely tend to the increase of 'peace on earth, good will to men.' This work, when finished, will be the realization of the ardent wish of statesmen and philanthropists everywhere, the dream of kings and conquerors during the last three hundred and fifty years, and a fitting supplement to the grand achievements which have marked the progress of the nineteenth century.'

"The principal engineering works of Mr. Eads have now been very briefly sketched. To speak of them at any length would require too much space, although much more that is instructive and delineatory of Mr. Eads' character could profitably be stated.

"In addition to these more important undertakings, he examined and reported upon many engineering projects.

"In March, 1878, at the request of the city council of Jacksonville, Florida, he made a report upon the practicability of deepening the channel through the bar at the mouth of the St. John's river.

"After the construction of the South Pass jetties he was requested by the people of Galveston, Texas, to formulate a plan and take a contract from the United States Government to improve that harbor. He gave long and careful attention to the project, but Congress was not willing to grant a contract for the work.

"At the request of the Canadian Government he examined the harbor of Toronto and made plans for its improvement.

"At the solicitation of the Mexican Government he made surveys

and elaborate plans for the improvement of the harbors of Vera Cruz and Tampico.

"He examined into the problem of the drainage of the Sacramento river, as consulting engineer of the State of California.

"In 1884, by request of the Mersey Docks and Harbor Board of Liverpool, he appeared before a committee of the House of Lords and gave his testimony as to the effect of the terminal works of the Manchester ship canal upon the estuary of the Mersey and the bar at Liverpool. He brought to the solution of this question that same keen insight into hydraulics and the same close application that had made him so successful in this country. He showed so plainly the deleterious effects of the proposed plans that the committee decided against them. They have since been changed to conform to his suggestions.

"The Emperor of Brazil, Dom Pedro II, held Mr. Eads in high esteem, and made a special visit to the South Pass jetties when he was in this country. He afterwards offered Mr. Eads the position which was given, on his recommendation, to the lamented W. Milnor Roberts, past president Amer. Soc. C. E. About two years ago he was requested by the Brazilian Government to examine the mouth of the Rio Grande do Sul and make plans for its improvement, but ill health and pressing business prevented his acceptance of the offer.

"Mr. Eads traveled extensively in this and foreign countries and often in pursuit of information bearing upon the engineering enterprises which he had in hand.

"In 1877 Mr. Eads received a diploma from the University of the State of Missouri conferring upon him the honorary degree of LL. D.

"The letter of the president of the university, Hon. James S. Rollins, which accompanies the diploma, is worthy of being placed in this memoir, for it shows the appreciation in which Mr. Eads was held by those who knew him well in the State where the larger part of his life was spent: 'Allow me to say that this high honor has not been bestowed upon you as a matter of mere compliment to an eminent citizen of the State in which he resides, but on account of the high appreciation of your high position as a man of great scientific attainments and of the practical uses of this extensive knowledge in improving the facilities of commerce, developing the

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internal resources of the country, and adding incalculably to its wealth and to the happiness and prosperity of its people.

“ ‘The bestowal of such honors and the recognition of your great services in accomplishing the noble ends at which you aim will be some remuneration to you for the sacrifices and labors which you have undergone in achieving such great advantages for our common country.’

“ In 1884 the Albert medal was awarded to Mr. Eads, he being the only American on whom this distinguished honor has been conferred.

“ The letter of the Secretary notifying Mr. Eads of the award is given following, as well as the resolutions of the Society :

“ ‘SOCIETY FOR THE ENCOURAGEMENT OF ART,
“ ‘MANUFACTURES, AND COMMERCE,

“ ‘JOHN STREET, ADELPHI, LONDON, W. C., *June 10th, 1884.*

“ ‘SIR: I am directed by the Council of this Society to inform you that they have had the pleasure of awarding to you the Society’s Albert medal as a token of their appreciation of the services you have rendered to the science of engineering.

“ ‘The medal was founded in 1862, in memory of His Royal Highness the Prince Consort and is awarded annually for distinguished merit in promoting arts, manufactures, or commerce.

“ ‘I may add that the award is made with the approval of His Royal Highness the Prince of Wales, the president of the Society, to whom it has been submitted. Should you be in England I have no doubt His Royal Highness would be pleased to present the medal to you himself, as he has done in previous cases, but should there be no opportunity for his doing so he will arrange for its transmission to you.

“ ‘I inclose for your information a list of the distinguished men of science to whom the medal has been given in previous years.

“ ‘I have the honor to be, sir, your obedient servant,

“ ‘H. T. WOOD,

“ ‘*Secretary.*

“ ‘James Buchanan Eads.’

“ ‘The following are the precise terms of the award :

“ ‘*Resolved*, That the Albert medal be awarded to Captain James Buchanan Eads, the distinguished American engineer, whose works have been of such great service in improving the water communica-

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tions of North America, and have thereby rendered valuable aid to the commerce of the world.'

"Mr. Eads took just pride in this medal, for it had been conferred previously upon such distinguished men as Sir Rowland Hill, Napoleon III, Sir Joseph Whitworth, Ferdinand De Lesseps, Sir Henry Bessemer, Sir C. W. Siemens, Sir William Armstrong, Sir William Thompson, and several other illustrious persons.

"Mr. Eads was elected a member of the American Society of Civil Engineers December 16th, 1868, and a fellow March 30th 1870. He was vice-president from January 18th, 1882, to January 17th, 1883.

"He was elected a member of the Institution of Civil Engineers, Great Britain, May 4th, 1869, and fellow of the American Association for the Advancement of Science in 1879, and a member of the British Association in 1884. He was a member of the Society for Encouragement of Arts, Manufactures, and Commerce. He was also a member of the Engineers' Club of St. Louis and of the American Geographical Society. He was for two years president of the Academy of Science of St. Louis.

"The inventive genius of Mr. Eads is shown in the fact that nearly fifty patents were issued to him by the Governments of the United States and England for useful inventions in naval warfare, bridge foundations and superstructure, dredging-machines, navigation, river and harbor work, and ship-railway construction."

Mr. Eads was twice married. His first wife left him two daughters. His second wife was a widow with three daughters, and the combined families became as much one in love and accord as if the children had all been of the same parentage, and this fact speaks volumes for his lovable nature. He was a pleasant and forcible public speaker, but it was in private conversation that his persuasive eloquence, coupled with his mastery of the subject in hand, shone most conspicuously, and in this field he probably displayed greater ability than in devising the schemes which have given him a world-wide celebrity. He had a peculiar capacity, a sort of special instinct, for selecting the best men to aid him in the various departments of his great enterprises—men of special qualification for the parts assumed by them and of national reputation became his zealous coworkers—and he possessed this special characteristic of true greatness, that he never feared that praise of his associates might detract from his own fame. His views were broad, compre-

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hending in their scope enterprises of the greatest magnitude with the minutest details which were necessary to their success. He perceived the need before the necessity had arisen, and so was prepared with the relief when the time for supplying it had come. Once that need was perceived, be it great or small, he gave it his careful consideration to determine the value of its accomplishment and, being satisfied that the end was worth more than the cost of the attainment, he bent all of his energies to the work with a courage that knew not failure; hence it was that, although some of his projects were of the most stupendous character, he had so weighed their value and demonstrated to himself their feasibility that he was always able to enlist adequate forces for their accomplishment. With a mind thus constituted, success was not problematical; to him it was a demonstration; and he had that rare combination of qualities that enabled him to inspire others with the same conviction, so that, whether he lived to see the completion of his work or that his own end should be reached in advance of it, he had that sublime faith in its success that he could give his whole energies to it without a shadow to mar his efforts. He was of delicate and refined tastes, and especially fond of poetry, of which his retentive memory gave him large command. In social life he was always courteous and kindly, endowed with both wit and humor. He was a most agreeable companion. He had the happy faculty of adapting himself to those with whom for the moment he found himself, so that young and old, cultured and uncultured, enjoyed his society.

Perhaps few men are heroes to those who know them intimately, but those who were nearest to Mr. Eads honored and loved him most.

MEMOIR
OF
HENRY DRAPER.
1837—1882.
BY
GEORGE F. BARKER.

READ BEFORE THE NATIONAL ACADEMY, APRIL 18, 1888.

BIOGRAPHICAL MEMOIR OF HENRY DRAPER.

MR. PRESIDENT AND GENTLEMEN OF THE ACADEMY :

Of all the marvelous results which have been obtained by what has been felicitously styled the New Astronomy, perhaps none exceed in importance or in value those which have been obtained by the aid of photography. While the older or mathematical astronomy, so called, has had to do with positions and magnitudes and motions essentially, all expressible by the aid of suitable symbols in the form of more or less complex functions, the newer or physical astronomy has concerned itself largely with physical phenomena and appearances, the completest attainable record of which is essential to their successful interpretation. True, the contacts of an eclipse and the path of a transit, when registered by photography, have greatly aided the comparison of theory with observation in mathematical astronomy. But the value of photography here is inconsiderable when compared with that of the record which it can furnish of the lunar and solar surfaces, of the distribution of stars in a group, of the configuration of a nebula, or of the arrangement of lines in a solar or stellar spectrum. Indeed, the facility of reproduction by photographic means so far surpasses that by drawing or sketching, and is, moreover, so much more accurate a method of delineation, that the evidence given by an untouched photograph is everywhere accepted as *prima facie* proof. Among those who have assiduously devoted themselves to the evolution of astronomical photography in its many-sided relations, none, it will be conceded, has accomplished more, and none, therefore, more richly deserves the reward of full recognition from his fellow-workers, than our late member, Henry Draper, the accomplished investigator, scientist, and teacher, who is the subject of the present sketch.

HENRY DRAPER was born in Prince Edward county, Virginia, on the 7th of March, 1837. His distinguished father, John William Draper, who had come from England to the United States in 1832, had settled first in Christiansville, Mecklenburg county, and had at once begun there an active career of scientific research. In 1835

he entered upon a course of medical study in the University of Pennsylvania, and received the degree of Doctor of Medicine in 1836. Already, however, his original scientific papers had gained for him renown, so that in the fall of the same year he was elected to the chair of chemistry and natural philosophy in Hampden Sidney College, in Prince Edward county. This appointment it was which permitted him, in place of practicing his profession, to devote himself entirely to research; which enabled him, therefore, to continue the scientific investigations which he had begun and of which he was so fond, and which secured permanently to the cause of science in this country an investigator of the highest type. It was while he was thus acting as a professor in Hampden Sidney College that Henry was born. His mother, a daughter of Dr. Gardner, the attending physician of the Emperor of Brazil, Dom Pedro I, and a descendant on her mother's side of a noted Portuguese family, the De Piva Pereiras, was a woman of marked character and ability, and his earliest years were spent in an atmosphere of the highest scientific culture and refinement.

In the year 1839, having accepted the professorship of chemistry in the undergraduate department of the University of the City of New York, Dr. Draper moved thither with his family, Henry being at that time but two years old. As soon as he was sufficiently prepared, he entered the primary school, which was at that time connected with the University, from which he passed into the preparatory school. At the age of fifteen he entered the collegiate department as an undergraduate, where he was distinguished for excellent scholarship. By the advice of his father, however, and partly on account of his not very vigorous constitution, he did not remain to take the bachelor's degree with his class, but left the classical course upon the completion of the sophomore year and entered the department of medicine. Upon the termination of his medical studies in 1857, he passed all his examinations satisfactorily; but, not having attained the age necessary for graduation, his diploma was withheld. Thereupon, in company with his older brother, he went abroad for study and recreation, remaining an entire year. Upon his return in 1858, he received his medical degree, graduating with distinction. While in Europe he was appointed upon the medical staff of Bellevue Hospital, a position which he assumed after his return, and the duties of which he discharged for eighteen months. In 1860, at the age of twenty-three, he was elected Professor of Nat-

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ural Science in the Undergraduate Department of the University of New York, and in 1866 he was appointed Professor of Physiology in the Medical Department, being made at the same time Dean of the Faculty. This connection with the Medical School he retained until 1873. Subsequently he held the chair of Analytical Chemistry in the Academic Department of the University, and upon the death of his father, in January, 1882, he was chosen to succeed him as Professor of Chemistry, a position which he held, however, only until the close of the current academic year. He then severed entirely his connection with the University.

Reared in direct contact with science and scientific thought as Henry Draper was, it is not surprising that at an early age he developed a decided preference for scientific pursuits. The eminence of his father as a teacher, an author, a philosopher, and an investigator created about him an atmosphere of scientific culture of the highest tone. To live in contact with this genial and learned man was of itself an education of the greatest value. Henry was early taken into his father's confidence in all scientific matters, and was often permitted to assist his father, not only in his lectures, but also in his investigations. The scientific spirit which presses forward unflinching in the pursuit of truth and which wrests from nature the profoundest secrets by patient and long-continued application had always been characteristic of the elder Draper; it was now to be fully developed in his son Henry. While yet a student of medicine, he had undertaken a most difficult research upon the function of the spleen; and, conscious of the inaccuracies incident to drawings, he illustrated this memoir—afterward published as his graduating thesis—with photomicrographs of rare perfection for those early days, all of which were prepared and photographed by himself. It was while engaged with the microscope in making these excellent micrographs that he discovered the remarkable power possessed by palladium chloride in intensifying negatives, an observation which subsequently proved of much value in the photographic art. From this period dates his interest in photographic pursuits, in which he attained afterward such eminence.

During his sojourn in Europe in 1857 he attended the meeting of the British Association, which was held at Dublin in the month of August. Upon the adjournment of the meeting he was invited by the Earl of Rosse to join a party made up of the members for the purpose of visiting the six-foot reflecting telescope at Birr Castle,

Parsonstown. There he saw not only the great instrument itself, but also the machinery and methods by which it had been produced. The sight of this splendid telescope inspired him with a desire to construct a similar, though, of course, a smaller one, and was thus the means of turning his attention to astronomy and astronomical photography.

Accordingly, in September, 1858, shortly after his return from Europe, he began the construction of a Rosse machine for grinding and polishing a 15½-inch speculum. A preliminary trial on a smaller mirror, 8 inches in diameter and 11 inches in focal length, having proved satisfactory, the large speculum was successfully cast on the 2d or 3d of November. The casting turned out to be very fine, free from pores and of silvery whiteness. It was 2 inches in thickness and weighed 110 pounds. The speculum was to be of 12 feet focal length. Work was begun upon it on the 9th and on the 26th it was sufficiently advanced to permit of a preliminary test. On the 29th Jupiter's moons were seen with it with the naked eye. During the following winter this speculum was mounted as a Newtonian telescope, the flat secondary mirror having been made at the same time. The figure of the large mirror was not satisfactory, however, the machine showing a tendency to polish in rings of different focal lengths. Early in the spring of 1859, therefore, it was reground, this tedious operation being assisted by placing the mirror and grinder in a voltaic circuit, the space between the two being filled with acidulated water and the speculum being made the positive pole. In this way the speculum metal was attacked and removed by the electrolytic action, thus facilitating the grinding. If, however, the current was strong enough to make the operation of material value, it was found that the copper and tin were transferred bodily and were deposited upon the grinder. In consequence of this, another experiment was tried with this mirror. Certain defects having to be ground out, the depressions were covered with a thick layer of Canada balsam; and a thick rim of wax having been made around the edge of the mirror, nitrohydrochloric acid was poured on and the uncovered spaces were, in this way, quickly corroded away. Subsequently an increase of 15 inches in the focal length was secured by attacking the edge zones of the surface with the acid in graduated depths. The grinding and polishing of this speculum continued at intervals until the following summer, when the mirror was found to have a focal length of 11 feet 10½ inches,

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and to give fair results when tested upon the sun. In February, 1860, this speculum was found split entirely across, the parts forming two nearly equal halves, the result being due to the expansion in freezing of a few drops of water which had found their way into the supporting case.

In June, 1860, Prof. John W. Draper sailed for Europe. While abroad he visited Sir John Herschel, and by his advice wrote to Henry to abandon speculum metal and to make his mirrors of silvered glass, since, for an equal aperture, they were only one-eighth as heavy and at the same time reflected 93 per cent. of the light falling upon them. Accordingly a piece of glass 15 $\frac{1}{4}$ inches square and 1 inch in thickness was at once ground and polished. In November it was put into its tube, and during the month ten solar daguerreotypes were obtained with it. The following winter a new grinding-machine was contrived, and experiments were made with three 15 $\frac{1}{4}$ -inch glass disks, as well as with some smaller ones. Three mirrors of the same focal length and aperture, Dr. Draper says, are almost essential in working, for it not infrequently happens that two in succession will be so similar that a third is required for attempting to make any advance beyond them. One of the mirrors thus made was parabolic in figure and bore a power of 1,000, 77 hours and 10 minutes having been expended in grinding and polishing it. The three mirrors were all tested together in October, 1861. Number one was found to be nearly spherical, number two parabolic, and number three hyperbolic in form. Number two resolved ϵ Lyræ, separated the components of 36 Andromedæ, and, as Dr. Draper thought, divided γ^2 Andromedæ. Polaris was divided and stood easily a power of 1,000. After further time spent on these mirrors they were all pronounced good, and were consequently silvered.

The construction of these mirrors greatly interested Professor Joseph Henry, of Washington; and, believing that the practical details worked out so successfully by Dr. Draper would be of great value to others experimenting in the same line, he visited Dr. Draper's laboratory and observatory in the spring of 1863 and arranged with him to write a monograph upon the subject, to be published by the Smithsonian Institution. This paper was issued in July, 1864, as No. 180 of the Smithsonian Contributions to Knowledge. It has become the acknowledged authority on the subject, and has been several times reprinted.

In this memoir Dr. Draper treats of the methods of grinding and polishing the mirrors, the method of mounting them, the best forms of driving-clock, the construction of the observatory, the arrangement of the photographic laboratory, and the modes of photographic enlargement. "It is generally supposed," he says, "that glass is possessed of the power of resistance to compression and rigidity in a very marked manner. In the course of these experiments it has appeared that a sheet of it, even when very thick, can with difficulty be set on edge without bending so much as to be optically worthless. Fortunately in every disk of glass that I have tried there is one diameter on either end of which it may stand without harm." The glass used for these mirrors was such as is ordinarily employed for skylights, and had probably been subjected to a rolling operation when in a plastic state in order to reduce it to a uniform thickness; and hence the peculiarity referred to would seem to result from the structural arrangement of the glass itself. The same peculiarity has been observed, however, both in speculum metal and in optical glass, as has been proved by the observations of Short and by Airy. Many fine mirrors were condemned by Dr. Draper to be repolished before he appreciated the effect of position; mirrors which no doubt would have performed excellently had they been properly set in their mountings.

Again, the effect of temperature-changes upon the perfection of the form of the mirror is very remarkable. "A current of cold or warm air, a gleam of sunlight, the close approach of some person, an unguarded touch, the application of cold water injudiciously will ruin the labor of days." Of this effect he mentions the following example: "A 15 $\frac{1}{2}$ -inch mirror, which was giving at its center of curvature a very fine image of an illuminated pin-hole, was heated at the edge by placing the right hand on the back of the mirror at one end of the horizontal diameter. In a few seconds an arc of light came out from the image. On putting the left hand on the other extremity of the same diameter the appearance was that of two arcs of light crossing each other and having an image at each intersection. The mirror did not recover its original condition for ten minutes. Another person, on a subsequent occasion, touching the ends of the perpendicular diameter at the same time that the horizontal were warmed, caused the image to become somewhat like two of the former at right angles to each other. By unsymmetrical warming still more remarkable forms emerged in succession, some

of which were more like certain nebulae, with their milky light, than any geometrical figure. If the glass after one of these experiments had been immediately put on the polishing machine and repolished the changes in surface would, to a certain extent, have become permanent, as in Chinese specula."

Another propensity of glass was observed in these experiments. If the mirror be truly spherical it could give an image free from distortion only when so set that the object and image are both upon its optic axis. But Dr. Draper polished a large number of mirrors in which an image free from distortion was produced only when oblique pencils fell upon the mirror and the image was returned along a line forming an angle of from two to three degrees with the direction of the object. The explanation of this condition seems to be that the radius of curvature is greater along one of the diameters than along that at right angles. "How it is possible," he adds, "for such a figure to arise during grinding and polishing it is not easy to understand, unless it be granted that glass yields more to heat and compression in one direction than another."

After a description of the emery and the rouge needed for grinding and polishing the mirrors, and of the materials and construction of the tools used for the purpose, Dr. Draper goes on to describe the methods he employed for testing the surfaces produced. His habit was to test his mirrors exclusively at the center of curvature, not putting them in the telescope until parabolic or finished. "The means of trial are so excellent, the indications obtained so precise, and the freedom from atmospheric disturbances so complete that the greatest facilities are offered for ascertaining the nature of a surface." "I do not think," he says, "that anything more is learned of the telescope, even under favorable circumstances, than in the workshop." Two distinct methods of examination were employed, the second of which, due to Foucault, affords by itself a large part of the information required in correcting a concave surface. These are: "1st, observing with an eye-piece the image of an illuminated pin-hole at the focus, and the cone of rays inside and outside that plane; 2d, receiving the entire pencil of light coming from the mirror through the pupil on the retina and noticing the distribution of light and shade and the appearances of relief on the face of the mirror." After having obtained a spherical surface, Foucault moves the luminous pin-hole toward the mirror, at the same time retracting the eye-piece or screen, carrying it through a series of ellipsoidal

curvatures and thus advancing step by step toward a paraboloid of revolution. Inasmuch, however, as the focal length of Dr. Draper's mirrors was 150 inches, the use of Foucault's method would require a room more than 25 feet long. But, by studying the gradual increase of deformation produced by the greater and greater departures from a spherical surface as the parabola is approached, he so modified Foucault's method as to use it satisfactorily in a room of ordinary size. The principle of this modified method of testing is thus given: If a $15\frac{1}{4}$ -inch mirror of 150 inches focal length were spherical and were used to converge parallel rays, those from its edge would reach a focus $\frac{5}{100}$ of an inch nearer the mirror than those from its central parts. If, now, the converse experiment be tried, and a mirror of the same size and focal length, which can converge parallel rays falling on all its parts to one focus, be examined at the center of curvature, it gives there an amount of longitudinal aberration $\frac{10}{100}$ of an inch, equal to twice the preceding. This latter, then, is the condition at the center of curvature to which such mirror must be brought in order to converge parallel rays with exactness. This is literally a method of parabolizing by measure, and is capable of great precision when the eye learns to estimate where the exact focus of a zone is.

At the outset the machine employed for grinding and polishing was similar to that of Lord Rosse. Subsequently another machine was constructed based upon the principle of Lassell's grinder. With the experience thus gained, still a third machine was made, which gave better and more speedy results than either. Indeed, no fewer than seven different grinding machines were built and tested in the course of the experiments. The final machine, which was intended to give spiral strokes, was a simplification of Lord Rosse's. In it, however, the lateral motion was absent and a changeable stroke was required. Moreover, the mirror was always uppermost in polishing, and, being uncounterpoised, escaped the effects of irregular pressure to as great an extent as possible. Eventually, however, a method of producing reflecting surfaces by correcting these surfaces by local retouches was adopted as the best of all, the mirrors formed by it being as perfect as can be and yet requiring only a short time for their production. In applying this method, due originally to Foucault, several disks of wood are provided, varying in diameter from 8 inches to half an inch, covered with pitch or resin in squares on one side and having a cylindrical handle on the other. The mirror,

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already fined, is laid face upward upon several folds of blanket arranged upon a circular table. The large polisher is then moved over the surface in straight strokes upon every chord, the pressure being moderate. As soon as is practicable an examination is made at the center of curvature and the best diameter for support is marked. If the mirror is nearly spherical it is replaced and the polishing is continued for three or four hours, till a fine polish is obtained free from dots like stippling. The next step is to shorten the radius of curvature of the center zones so as to convert the section curve into a parabola. This is accomplished by straight strokes across every diameter of the face—at first with a 4-inch, then with a 6-inch, and finally with the 8-inch polisher. When critically examined, however, a wavy or fleecy appearance could be detected upon the surface, due to the unequal pressure of the hand in polishing; and, further, the change in the focal length became greatest on the edge. In consequence a polishing machine was contrived which entirely overcame these two faults.

For silvering the mirrors the process originally adopted made use of milk-sugar as the reducing agent, and was devised by Dr. Draper himself. Subsequently Foucault's method with oil of cloves was experimented with, and finally this gave place to the Rochelle-salt process, suitably modified. The mirror, warmed to 100° F., is placed in the silvering solution face down, and allowed to remain till the sun's disk appears through the film of a pale-blue tint. When dry it is rubbed with a piece of soft buckskin to condense the surface, and is then polished with rouge on buckskin. The thickness of the layer is about 1-200000 of an inch, the weight of silver on a 15½-inch mirror being less than 4 grains.

The observatory in which the reflecting telescope of Dr. Draper was mounted was constructed during the spring of 1860, on ground belonging to his father's estate, at Hastings-on-Hudson. It is located on the top of a hill, 225 feet above low-water mark, and is in latitude $40^{\circ} 59' 25''$ N. and longitude $73^{\circ} 52' 25''$ W., as determined by the Coast Survey. The observatory proper is $17\frac{1}{2}$ feet square, and is two stories in height, one being above the ground, and the other, equal in height, being excavated out of the solid granite upon three sides, the fourth being toward the east and open. To the upper portion and upon the southern side a photographic laboratory, 9 by 10 feet, was attached in 1862. The mounting of the 15½-inch telescope was on the alt-azimuth plan, but arranged (as first

suggested by Miss Caroline Herschel) so that the eye-piece or place of the sensitive plate is stationary at all altitudes. An ingenious system of counterpoise levers allowed the telescope to work in a space a little more than its own length across. The mirror rests upon an air cushion, supported upon a disk of oak carried by three flat iron bars secured to the lower end of the telescope tube, the whole being surrounded with a curtain of black velvet. In form the telescope is a Newtonian instrument. For photographic purposes the eye-piece of the telescope was provided with a sliding plate-holder, having a motion in the apparent direction of the moon's path. The advantages of such an arrangement are twofold: In the first place, it gets rid completely of the difficulties arising from the moon's motion in declination; and, in the second, it requires the motion of a mass of scarcely more than an ounce in place of that of the entire telescope—a ton or more. To move the plate-holder, a sand-clock was first employed. The weight in this clock rested upon a column of sand, which could be allowed to run out through an orifice variable at will. Subsequently a form of clepsydra or water-clock was contrived, with which the most satisfactory results were obtained. For solar photographs, an angular spring shutter was used, attached to the eye-piece, and movable past the opening by means of a stout rubber band.

During the winter of 1862 a large number of solar photographs were taken with the new telescope, upon both daguerreotype and tannin plates. In April several photographs of the moon were obtained on dry plates with exposure varying from five seconds to five minutes. In the fall of the same year mirror No. 3 was re-ground, and twenty hours were spent in polishing it. "It is as good as we know how to make it," he writes. "During the construction of this mirror nearly a hundred other mirrors, ranging in size from $\frac{1}{4}$ inch up to 19 inches, have been made and polished," he tells us. This mirror separated γ^2 Andromedæ and showed the color of the components. It also showed the companion to Sirius, and the sixth component of θ Orionis. Debillissima between ϵ and δ Lyre was proved quintuple, the moons of Jupiter gave beautiful disks, and the planet was covered with belts up to the poles. In the summer of 1863 lunar photography was again taken up, and the best photographs of the moon ever taken by any one up to that time were obtained during the first week in August. In all about 1,500 negatives of the moon were taken with this telescope. They were about

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an inch and a quarter in diameter and bore enlargement to three feet, and in one case to fifty inches, with excellent results.

In the fall of 1867 Dr. Draper began the construction of a glass mirror of 28 inches aperture and $12\frac{1}{2}$ feet focal length, which was to be mounted as a Cassegrainian telescope. Upon this work much of his spare time was spent for eighteen months, the mirror having been ground and polished 41 times up to the first of April, 1869. In June, after $17\frac{1}{2}$ hours of polishing, it was completed and tested. The edge-focus was outside of the center-focus about $5\cdot32$ of an inch, so that its figure was about parabolic. Its focus was found to be 148 inches, that of the smaller and secondary mirror being 29. The latter was to be placed 33 inches inside of the focus of the large mirror. During the summer of 1869 another dome was added to the Hastings observatory, and toward the close of August the new mirror was mounted in it. The secondary mirror was silvered, and Saturn and Arcturus were observed with a power of about 120. In October the mirror was supported on a cushion of rubber, and although unsilvered it showed the companion to Polaris. During the following winter the driving clock was completed, and in July, 1871, the large mirror was successfully silvered by Browning's process. On August 1st the first photograph was taken with the new mirror. It was of the moon, the image being five inches in diameter. In September the equatorial clock was attached, and proved so perfect that Saturn could be examined with a power of 2,000. The defining power of the 28-inch mirror not being equal to that of the 15-inch, it was concluded to repolish it, and this was done the following winter, so that in March it was regarded as virtually perfect. The accessories were then finished, and by the end of June, 1872, the large telescope was pronounced to be in complete working order. Twenty or more photographs of the moon were subsequently taken, requiring an exposure of ten seconds for the full moon and of twenty-five seconds when the moon was five days old. These photographs also were five inches in diameter. Shortly afterwards a flat secondary mirror of 16 inches diameter was constructed for use with the large mirror, in order to reduce the magnifying power.

In 1873 this telescope was used part of the time as a front view or Herschelian and part of the time as a Newtonian. With the former arrangement the full moon was photographed with an exposure of one-quarter second, and the image of Mars appeared quite round and free from tails. During August the moon was photo-

graphed on every clear night with an exposure of one second, and the pictures obtained were regarded as in every way the equals of those taken with the 15 $\frac{1}{4}$ -inch mirror. With the Newtonian arrangement Saturn, ϵ Lyræ, and α Lyræ were examined and the two doubles were satisfactorily seen, as well as the five components of Debillissima. The work done with this telescope consisted (1) of photographs of spectra of the midday and setting sun to determine the atmospheric lines in the photographic spectrum, the result of which was negative; (2) of a repetition of J. W. Draper's experiments showing protection at the upper as well as the lower portion of the spectrum, using a quartz lens of 14 inches focus and one or two quartz prisms of 60° (the protected portion appeared to come down into the photographic region proper); (3) of an examination of the spectrum and a photograph of Coggia's comet; and (4) of a study of Saturn and his system in conjunction, first, with Professor Newcomb and afterwards with Professor Holden. In 1874 the reflector was changed again to the Cassegrainian form. Inasmuch as it had never been ascertained whether or not this mirror had a best diameter of support, it was taken to New York in 1876 and tested. This was found to be no easy matter, as the least pressure in the way of wedging deformed the mirror more than turning it round, and great care was necessary in centering it. After three nights of measurement Dr. Draper was satisfied that such a diameter existed. In two positions a single image was given, while in two others at right angles to these a completely double image was produced. In consequence of these results a system of support by means of back levers was decided upon. Dr. Draper concludes: 1st, that he is fully satisfied that there is an axis of maximum rigidity, and that the nature of the support influences the direction in which it should be placed; 2d, that even with the best India-rubber band support there is not as good definition as when the weight of the upper part of the mirror is supported by edge levers; and, 3d, all things considered, Lassell's plan of independent systems of levers is the most feasible. The general idea was to cement 18 squares of glass to the back of the mirror, to drill a round hole through each, and to put in each hole the end of a lever whose fulcrum shall be in the terminus of a tertiary back lever. Suitable weights on the rear ends of these edge levers will carry the weight of the mirror. This apparatus was completed in the spring of 1876, and in July the photographic stellar spectrograph was attached to the reflector and

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several spectra of Vega were taken with it. "By the word spectrograph," he says, "we indicate the apparatus for photographing stellar spectra. It consists of a box about 3 feet long, which screws into the back of the reflector, and contains in the order from before backwards the following parts: 1st, a slit about 1 inch long, the same that I used for my diffraction photography. Close behind this is the brass tube of the Browning direct vision spectroscope, which contains either 3, 6, or 9 prisms, at will. Behind this and at a distance of 14 inches from the slit is the 7-inch Voigtlander portrait lens; behind this comes an ordinary pocket camera." Various photographs of the moon and of Jupiter were also taken with the 28-inch mirror. In July, 1880, it was again silvered, this time by Martin's process, and a series of comparative experiments was made in taking various stellar spectra and in securing photographs of the moon. At Professor Henry's request, Dr. Draper began the preparation of a description of this telescope and the methods employed in its construction, to be published as a supplement to his early memoir on reflecting telescopes; but this paper was never completed.

Desiring to compare the performance of a large refractor with the 28-inch reflector, Dr. Draper ordered from Alvan Clark & Sons, in the winter of 1875 (February 25), a refractor of 12 inches' clear aperture. In September the object glass was received, and soon afterward was mounted on the same axis with the 28-inch reflector, the eye-pieces at the same level. About the middle of October eleven photographs of the moon were taken upon the same evening, nine with the refractor and telescope clock and two with the reflector and lunar clock. The exposure was 20 seconds for the reflector, the secondary mirror being somewhat dull; the exposure in the refractor was less than 5 seconds. Since the areas of the pictures were as 9 to 1, that of the objectives being as 5 to 1, the time of exposure, assuming that the reflector reflects as much as the refractor transmits, should be about as 2 to 1. They were, in fact, as 4 to 1. The photographs were about equal in goodness; the terraces in Copernicus and the multiple cones were visible in both. By means of a Browning-Huggins spectroscope the distance of the actinic focus outside of the visual in the refractor was determined by the method of Rutherford and found to be, in the neighborhood of G, 62-100 of an inch. The focus for H was an inch outside the focus for C. The difference between the spec-

trum of a star in the two kinds of instrument is also noted. In the reflector the star-spectrum is parallel-sided, while in the refractor it is parallel only from about D to F, and above and below these points it spreads out into a brush. In June following comparisons were instituted between the two instruments on Venus and Jupiter. Venus in the reflector was without color and Jupiter appeared silvery white, the belts being of a brownish or brownish-yellow tint. In the refractor the claret color and green shown upon Venus are stated to be very annoying, and the belts of Jupiter appeared to be dusky upon a pinkish background. In July the refractor was furnished with a silvered Rutherford-grating spectroscope and the solar and lunar spectra were examined, as well as those of Jupiter, Vega, etc. The solar protuberances were also examined, one of these observed on July 26 being 1' high. In August, 1877, the satellites of Mars were observed by Dr. Draper and Professor Holden with the refractor. In the spring of 1880 this refractor was exchanged for an 11-inch achromatic, made by the Clarks for the Lisbon Observatory. This instrument was rendered additionally valuable by the fact that it was provided with a photographic corrector. The new telescope was mounted in the place of the 12-inch instrument, and in 1880 the first photograph of the nebula of Orion was taken with it.

The scientific reputation of Henry Draper, in all probability, will rest chiefly upon his photographic investigations:—First, upon the diffraction spectrum of the sun; second, upon stellar spectra; third, upon the existence of oxygen in the sun; and, fourth, upon the spectra of the elements.

Because of the much greater suitability of the diffraction spectrum for the most accurate scientific work, Dr. Draper deemed the preparation of a trustworthy map of those parts of the solar diffraction spectrum which can be photographed on collodion the most suitable introduction to his investigation upon the spectra of the fixed stars, especially if to such a spectrum an accurate scale were attached for reading directly the wave lengths of the rays. Until the researches of J. W. Draper upon the diffraction spectrum, it had been supposed that three distinct and different types of energy existed in three different but overlapping spectrum regions. Heat was supposed to lie principally at the less refrangible end, light in the middle, and actinism at the more refrangible end. The elder Draper, however, showed that this error has arisen in part from

using prismatic spectra for investigation—spectra in which the red end is unduly condensed and the violet unduly dilated, in consequence of which the rays are not presented in the true order of wave lengths; and also in part from the nature of our ordinary photographic substances. The power of chemical decomposition he proved not to belong to the violet end of the spectrum as such, but to exist in every part of it. Bromide and iodide of silver, however, the substances which are used in collodion photography, turn out to be more readily decomposed by vibrations of certain periods than of others, and hence the excess of action seen at the violet end is a function of certain silver compounds and not of the spectrum. The solar beam is, therefore, not compounded of three distinct forms of energy—light, heat, and actinism; it is a series of æther vibrations, which may give rise to any one of these manifestations, depending on the surface on which it falls. Hence the normal character of the diffraction spectrum, in which no portion is unduly extended or compressed and in the two symmetrical halves of which, as J. W. Draper himself first proved, the energy is precisely the same. Distance in such a spectrum is almost exactly proportional to wave length.

The earliest photograph of a diffraction spectrum was taken by J. W. Draper in 1843. The earliest work in diffraction spectra by Henry Draper was in the fall of 1869, when he adjusted his lathe so as to rule steel and speculum metal gratings with 3,600 and 7,320 lines to the inch. In January following he ruled a concave speculum of 18 inches in focus, so that the rays themselves, without telescope or lens, might form an image; and found it to work well. In the fall of 1872 he renewed his experiments; and, having secured from Mr. L. M. Rutherford three of his superb glass gratings, ruled, respectively, with 4,320, 6,480, and 12,960 lines to the inch, he began a series of experiments to determine the best conditions of adjustment and of accessory apparatus. The sunlight was reflected from the secondary mirror of a heliostat upon a slit about 1.50 of an inch in width. Behind this, at a distance of 14 feet, the 6,480 grating was placed, the divided side turned from the slit. Then close to the grating came an achromatic lens of 5 inches aperture and 78 inches focus, set perpendicular to the mean ray; and, finally, the sensitive plate, about 12 inches long, in order to take in the spectrum from G to O. The violet rays came to a focus about 2 feet nearer to the grating than the red ones. In order to

adjust the position of the plate he made many experiments, and finally found that the plane of the plate must be parallel to the plane of the grating when the lens is perpendicular to the outgoing beam. The spectrum of the third order was employed, and by these adjustments he had a plate in focus from G to P, and visually *b* of the third and fourth spectra in one plane. The spectrum of the third order was selected for the photographs because of two conspicuous advantages which it possesses. In the first place, it is dilated to such an extent as to give a long image, and yet not one too faint to be copied by a reasonable exposure of the sensitive plate; and, in the second place, the spectrum of the second order overlaps it in such a way that D falls nearly upon H and *b* upon O. These coincidences are of service in determining the true wave lengths of all the rays.

In order to provide against an excess of action in certain parts of the spectrum, a system of diaphragms was introduced in the vicinity of the sensitive plate and removed at suitable times during the exposure. The region from wave length 4,000 to 4,350 requires only about one-tenth of the time demanded by that from 3,440 to 3,510. In one of the best of the photographs taken, the line O had an exposure of 15 minutes, while G had $2\frac{1}{2}$ minutes; and yet the former is underexposed. If these exposures seem exceptionally long for a wet collodion plate, it must be remembered that the slit was only 1-110 of an inch wide, and that the grating gives an almost complete circle of spectra round itself, this thin band of light being divided between these spectra. A beam 1-110 inch wide, therefore, is spread out in this case into a streak about 78 feet long.

Accompanying Dr. Draper's memoir is a plate of the diffraction spectrum of great excellence, transferred from the original negative directly to glass and printed as from a lithographic stone. Being absolutely free from retouching it represents faithfully the work of the sun itself. In the upper portion of the plate a spectrum is shown giving all the lines from G to O; *i. e.*, from wave length 4,350 ten millionths of a millimeter to 3,440. Above that is placed a scale which is a copy of Angström's from just below G to H_2 , with the same sized divisions carried out from H_2 to O. The lower part of the plate shows a magnified portion of the same negative having H_1 and H_2 about its middle, and extending from wave length 4,205 to 3,736. The great superiority of the photograph over a drawing, even in the visible regions of the spectrum, is shown by comparing

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the portion of the more refrangible region taken from Angström's chart and printed on the plate with the photograph beneath it. Between wave lengths 3,925 and 4,205 the drawing shows only 118 lines, while the original negative shows 293 at least.

In order to fit a scale to the photograph, advantage was taken of the fact that the second spectrum overlaps the third, the ray D of the second being near H of the third and b of the second near O of the third. Two fine steel points were placed, one in the position of D_2 of the second order and the other in that of b_1 of the second order. On developing the picture two sharply defined images of the steel points appeared on the spectrum; that of the point coincident with D_2 of the second order was found to have cast its shadow on H_2 of the third order, and that of the point at b_1 upon the spectrum of the third order near O. Hence a given ray in the composite H_2 has a wave length 3,930 ten-millionths of a millimeter, and one near O of 3,444.6. Since the ray G of the third order, the wave length of which is known, was photographed directly, three fixed points of reference in the photograph, of known wave length, have thus been secured. In order to produce and apply a scale to the photograph reading to ten-millionths of a millimeter, correct for these fixed points, the scale was ruled with a linear dividing engine and photographically reduced to fit the spectrum.

In speaking of this adjustment Dr. Draper says: "It is proper in this place to make a criticism on my scale and to point out a small error which may be due, however, to an incorrect determination of the wave lengths that I have used as fixed points." Taking the distance from G (wave length 4,307) to H_2 (3,930) and dividing it into 317 parts, and then prolonging these divisions toward O, he found that the third fixed point was not attained, the error being about two divisions. It is interesting, in proof of the scrupulous accuracy of Dr. Draper as an investigator, to note certain results published by Professor Pickering in 1886, obtained by comparing the wave lengths of the ultra-violet lines, as deduced from Dr. Draper's photograph, with those obtained from Professor Rowland's superb spectrum, taken thirteen years later with greatly improved apparatus and on a much larger scale. The positions of seventy-six lines on Dr. Draper's spectrum were read, estimating to tenths of a division by the aid of a lens; the same lines were then read from Dr. Rowland's map, their positions being also estimated to tenths. In this way a table of corrections was obtained, giving the

value to be added to or subtracted from the wave lengths as given by Dr. Draper's map, to produce the true wave length. These systematic corrections being made, Professor Pickering shows that the mean difference for the seventy-six lines compared is 0.012, and corresponds to about one-eight-hundredth of an inch on Dr. Draper's map. "We may therefore assume," he concludes, "that the probable error of a wave length derived from the map of Dr. Draper will not exceed one-hundredth of a unit if the correction above given is first applied. The minuteness of this quantity is a good illustration of the accuracy obtainable from a record obtained automatically by photography." Since the divisions on Dr. Draper's scale represent ten-millionths of a millimeter, this corresponds to a probable error of only one-tenth of a single scale division.

This memoir of Dr. Draper's was received very cordially in Europe. Secchi reproduced the spectrum on steel and introduced it into his monograph upon the sun. In 1880 a lithographed copy of the plate was published in the proceedings of the British Association as the most suitable reproduction known for the purpose of determining the wave lengths of the fixed solar lines.

Stellar spectrum photography was a subject which, at least in this country, Dr. Draper had made entirely his own. In May, 1872, as soon as his great reflector was completed, he made a trial of it for this purpose, and succeeded in securing at this time his first spectrum of a fixed star. This star was Vega (α Lyrae), and the photograph of its spectrum was obtained with a quartz prism, placed just inside the focus of the small mirror, no slit or lens being used. On the 8th of August following, however, a much better photograph was secured of the spectrum of this star, showing four lines very distinctly, the least refrangible being near G. In order to secure width in the spectrum, the telescope was given a slight motion in declination during the exposure. On the 31st of August, 1873, he photographed the spectrum of α Aquilæ with the same apparatus, the photograph being half an inch long and one-thirty-second of an inch wide. Though the exposure was ten minutes, it does not appear that this spectrum showed any lines. Subsequently, the quartz prism was combined with a cylindrical lens, but the result was not satisfactory, since it gave the spectrum the form of an elongated image of the mirror instead of a narrow band. Afterwards the quartz prism was combined with a heavy flint-glass prism of 60° , thus forming a nearly direct-vision spectroscope, having very good

dispersion. With this apparatus he obtained easily the spectrum of α Aquilæ as a narrow band in five minutes.

Such a research as the present one was beset with many and serious difficulties. In its earlier stages wet collodion plates were used, and the limitations imposed upon the time of exposure, in consequence, rendered it almost impossible to get strong impressions. Even then these impressions had no value for measurement, and were preserved generally by stripping the films from the plates and gumming them into the note book. With the use of the dry-plate process in 1879 these difficulties for the most part disappeared. The development of the dry plates was generally produced by ferrous oxalate, although the alkaline development and pyrogallic acid were also both used. Another difficulty encountered in such work is that of securing a sufficiently accurate movement of the driving clock, since any error here increases with the time of exposure. Dr. Draper was obliged to construct no less than *seven* driving clocks before he succeeded in getting one that was perfect. The regulator of the last one was a heavy conical pendulum, or rather pair of pendulums, weighing some fifteen pounds, and so hung that their revolutions were sensibly isochronous through quite a range of inclination. Whenever by increase of driving power or decrease of resistance one of the balls rose above a certain limit it acted, without affecting the radial motion of the ball, upon a friction spider which absorbed the superfluous energy. This regulator revolved once in a second. The gearing and driving screw were constructed, for the most part, by Dr. Draper himself, with the utmost care and accuracy; and, according to Professor Young, it may safely be said that in its ultimate perfected condition the driving clock was as good as any in existence, keeping a star upon the slit for an hour at a time when near the meridian and not disturbed by changes of refraction.

Other difficulties, which Dr. Draper was obliged more or less completely to overcome, related to the firm and rigid connection of the parts of the spectroscope with each other and with the sensitive plate; to the effect of temperature not only upon this connection, but also upon the dispersive power of the prisms employed; and, finally, to the mode of obtaining satisfactorily a reference spectrum for comparison with that of the star under examination. Then, too, the difficulties due to atmospheric conditions cannot be lost sight of. Photographic operations are much more sensitive to these conditions than are eye observations. A slight haze, which is sometimes

even an advantage for ordinary telescopic work, cuts off the active rays so decidedly that the time of exposure must be greatly increased. On evenings apparently good an exposure of half an hour or an hour is needed to secure an impression which may be obtained on other evenings in five or ten minutes.

In this research quite a variety of forms of spectroscopic apparatus was employed. In the earlier photographs a quartz prism only was used, as above mentioned, this prism being placed in the path of the rays a few inches within the focus, and with this spectroscope the first stellar spectrum was taken which showed lines. Afterwards Dr. Draper used direct-vision prisms in the same way, and also spectroscopes made of such prisms, in some cases with a slit and in others without, the necessary width of the spectrum being secured frequently by the aid of a cylindrical lens. The apparatus finally decided upon, and with which all the later spectrum work was done, was a Browning star spectroscope having two 60° prisms of dense but white flint glass of the form suggested by Dr. Huggins for stellar observations. The telescope and collimator each had a focal length of six inches and an aperture of three-fourths of an inch, the parts being well braced to secure firmness. A diaphragm was placed over the slit, the opening in which was surrounded with a ring of phosphorescent paint to render it luminous in the dark. A movable finger enabled any part of the slit to be exposed at pleasure for the purpose of obtaining reference spectra upon the same plate. The eye-piece and micrometer were removed from the eye end of the observing telescope of this spectroscope and a piece of hard wood was fitted on at this end to carry the photographic plate. This was very small, only about an inch square, cut from a plate of ordinary commercial size. A small positive eye-piece was attached to the block in such a way that the operator could examine at pleasure the yellow and red portion of the spectrum which projected beyond the sensitive plate into the field of view, and so satisfy himself that the entire apparatus was working properly. The photographs which were obtained with this arrangement were about one-sixteenth of an inch in width and about half an inch long, extending from a point between the Fraunhofer lines F and G to a point near M.

The stellar-spectrum photographs taken in 1872 and 1873 have been already mentioned. In the summer of 1874 preparations for the expedition to observe the transit of Venus occupied all Dr. Draper's leisure time; so that it was not till the fall of 1875 that

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stellar-spectrum work was again resumed. In October of this year several photographs of the spectrum of Vega were taken, a Browning nine-prism direct-vision spectroscope being used without a slit, placed inside the focus of the 28-inch mirror, the sensitive plate being at the focus. In July, 1876, Dr. Draper contrived an apparatus which he called a "spectrograph," with which he took several photographs of the spectrum of Vega. This apparatus consisted of a box about three feet long, which screwed into the tail-piece of the reflector in place of the eye-piece, and was made up of the following parts: 1st, a slit; close behind this the brass tube of a Browning direct-vision spectroscope, containing either three, six, or nine prisms, at will; next to this, and fourteen inches from the slit, a 7-inch Voigtländer portrait lens and camera. The results were not materially different from those obtained by the earlier methods, and the apparatus was cumbrous and awkward to manage. About this time Dr. Draper introduced the plan of setting the slit in the direction of the right ascensional motion; so that any slight irregularities of the driving clock would only widen the spectrum a little instead of removing the star's image from the slit. He had already, during the preceding fall, experimented with a "Registering Transit Spectroscope," suggested by Dr. J. W. Draper, the idea of which was to drive the observing telescope of the spectroscope by clock-work and to register on the chronograph the transits of the Fraunhofer lines. The chief difficulty encountered was the fact that no two observations gave the same reading when the lines of the solar spectrum from *a* to F were observed—a result due to irregularities in the wheel-work of the clock and the shape of the teeth. In the hydrogen spectrum the readings from C to F were found to be liable to an error of four seconds of time in 110 seconds on the chronograph. By making the clock of the spectroscope break the circuit of the chronograph, instead of the chronometer, the error was reduced to 0.4 of a beat in 148.3 beats; corresponding to an error of one part in 370 in the distance between C and F. By lessening the speed of transit the error would be still further reduced.

In the early fall of 1876 the Browning direct-vision prism was placed, without slit or lens, inside the focus of the 12-inch refractor. The apparatus was so arranged that a cylindrical lens of 14-inches focus could be placed either between the prism and the object-glass or between the prism and the sensitive plate. By slightly tilting the plate the difficulty arising from the differences in the foci of the

different rays produced by the object-glass was in great measure obviated. In October the Huggins star spectroscope was employed with one prism. It was attached to the refractor and used at first with the slit wide open. The collimator required to be accurately adjusted in line with the optical axis of the refractor. Another stellar spectroscope was arranged a few days later, consisting (1) of a slit with a wide open space between it and the end of the telescope, so that the centering of the star on the slit could be observed; (2) of the nine-prism Browning direct-vision combination; (3) of two opera-glass lenses; and (4) of the plate-holder. The several stellar spectra photographed with this apparatus turned out, however, to be rather faint. During the same month experiments were made upon the spectrum of Venus, using both the reflector and the refractor, the stronger pictures being given by the former. They show the lines well, especially those between G and H. On the 29th the aperture of the refractor was reduced to $1\frac{1}{2}$ inches, the same stellar spectroscope was attached to it, the slit was closed so that in the solar spectrum *b* appeared distinctly triple, and a series of photographs was taken with exposures of four minutes, one minute, five seconds, and one second, the last being the most suitable for the purpose. Compared with the six photographs of the spectrum of Venus taken on the 25th, these results indicated the proper exposure for Venus to be 196 times that of the sun.

Pressure of scientific work in other directions caused an intermission in Dr. Draper's stellar-spectrum investigations. On his return from Europe in July, 1879, however, this work was again taken up and prosecuted with vigor. In a paper read in October before the National Academy of Sciences, Dr. Draper thus gives a description of his apparatus: "The especial spectroscope for stellar work that is now on the telescope is intended to satisfy the following conditions; 1st, to get the greatest practicable dispersion with the least width of spectrum that will permit the lines to be seen; 2d, to use the entire beam of light collected by the 28-inch reflector or 12-inch achromatic without loss by diaphragms; 3d, to permit the slit to be easily seen, so that the star may be adjusted on it; 4th, to avoid flexure or other causes that might change the position of the spectrum on the sensitive plate in pointing the telescope first on one and then on another object; 5th, to admit of observing the spectrum on the sensitive plate at any time during an exposure without risk of shifting or disarrangement. The dispersion is produced by two

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heavy flint prisms which are devoid of yellow color; the telescopes are about six inches in focal length and the slit has a movable plate in front of it, enabling the operator to uncover either the upper or the lower portion at will."

With reference to the telescopes Dr. Draper says: "There is an advantage possessed by the refractor for this work which does not appear at first sight. Naturally one supposes that a reflector which brings all the rays from the star, no matter what their refrangibility, to a focus in one plane would be best, because when the slit is put in that plane it is equally illuminated by rays of all refrangibilities, and the spectrum will be parallel-sided in its whole length. On the other hand, a refractor is not achromatic, for the violet end of the spectrum comes to a focus either inside or outside of the plane of the rays in the middle of the spectrum, and in observing the spectrum it is not parallel sided. It is easy, therefore, with a refractor so to adjust the position of the slit that you may have a spectrum tolerably wide at F and G, and which gradually diminishes in width toward H, and finally becomes almost linear at M. Now, as the effect of atmospheric absorptions on the spectrum increases as you pass from G towards H and above H, by diminishing the width of the spectrum you can in some measure neutralize the effect, and at one exposure obtain a photograph of nearly uniform intensity from end to end, though it is of variable width. If it were not for this, it would be necessary to have the spectrum overexposed at G in order to be visible above H, or else to resort to an elaborate diaphragming, which is difficult." However, he announces his intention of returning to the use of the 28-inch reflector because it collects nearly five times as much light as the 12-inch refractor, after making allowance for the secondary mirror.

Of the results obtained up to that time Dr. Draper thus speaks: "A preliminary examination shows at once that these stellar spectra are divisible into two groups—first, those closely resembling the solar spectrum; and, second, those in which there are relatively but few lines and these of great breadth and intensity. The photographs of the spectra of Arcturus and Capella are so similar to the solar spectrum that I have not up to the present detected any material differences. But, on the other hand, the spectra of Vega and α Aquilæ are totally different, and it is not easy without prolonged study and the assistance of laboratory specimens to interpret the results, and even then it will be necessary to speak with diffidence."

"In the case of the spectrum of Vega when examined by the eye the lines C, F, near G and *h*, are readily visible, but lines such as D and *b* are relatively faint. It is clear, then, that hydrogen exists, to a large extent, in the atmosphere of that star; but, on examining the photograph of its spectrum, it is evident that other lines just as conspicuous as the hydrogen lines are present. One of these corresponds in position and character to H_1 , and seems to coincide with a calcium line. It appears to me, however, that the evidence of this coincidence is not complete." "I have not as yet obtained any stellar-spectrum photographs belonging to the third and fourth groups of stellar spectra, as described by Secchi. These, if obtainable, will aid materially in discussing the whole subject; but, unless a star passes near the zenith, it is hard to make a fair study of its spectrum by photography, because atmospheric absorption in the ultra-violet region increases rapidly as the altitude decreases."

An excellent account of Dr. Draper's "Researches on Astronomical Spectrum Photography" by Professors C. A. Young and E. C. Pickering was presented to the American Academy of Arts and Sciences in April, 1883, and is published in its Proceedings. In this paper a list is given in tabular form of all the photographs of planetary and stellar spectra taken subsequent to August, 1879, which were thought by Dr. Draper worthy of preservation. These photographs are seventy-eight in number, all taken on dry plates with the two-prism Huggins star spectroscope, used in some cases with the reflector and in others with the refractor. The first column in these tables is one of reference numbers. The second gives the date of the photograph, the third the object giving the spectrum, the fourth the local mean time of beginning the exposure, so far as given in Dr. Draper's note books, the fifth the duration of the exposure, the sixth the width of the slit, the seventh the aperture of the telescope used, and the eighth gives remarks from the note books concerning the particular photograph in question. Twenty-one of these spectrum photographs were measured by Professor Pickering with the micrometer of the Harvard College Observatory, the work done upon them being divisible into three parts: First, the determination of the relative positions of the lines in the various spectra in terms of any convenient unit of length; secondly, from the known spectra of the moon and Jupiter, a determination of the relation of these measures to wave lengths; and, thirdly, a reduction of the measures of the stellar spectra to wave lengths and a discussion of the results.

The stars whose spectra were thus measured were α Lyræ, α Aquilæ, α Aurigæ, α Bootis, and α Scorpii; comparison spectra of the moon or Jupiter being photographed in some cases upon the same plate. In all, one hundred and twenty-seven lines were measured in the spectra of these five stars and their wave lengths determined by comparison with Dr. Draper's diffraction spectrum taken as a standard. The values for α Aquilæ and α Lyræ are shown to agree well together and with the results of Dr. Huggins' measurements. The distinctive feature of these spectra is a series of broad dark lines at regular intervals, the lines of greater wave length appearing to coincide with the hydrogen lines. The interval between the successive lines diminishes with the wave length. The spectra of α Aurigæ and α Bootis resemble that of the sun, their lines coinciding with the solar lines, not only in position, but also in intensity. "Of the twelve lines seen in at least seven of the nine spectra of the moon and Jupiter, every one is contained in the spectra of both α Aurigæ and α Bootis. Of the fifteen lines which are so faint as to be contained in but one or two of the spectra of the moon or Jupiter, only four are contained in the spectrum of α Bootis and but one in that of α Aurigæ. The evidence afforded by these photographs, therefore, points very strongly to the conclusion that the spectra of these stars, and consequently their constitution, are the same as that of our sun." In the spectrum of α Scorpii twenty-five lines were measured. A comparison of all the results showed that the plates used were sensitive to light of wave lengths 3,750 to 4,800—that is, from M nearly to F, although when the light was intense the spectra extended beyond these limits.

It would seem fitting to mention here the liberal plans which Mrs. Draper has made to carry on the work of her lamented husband in astronomical spectrum photography. Already, in 1885, appropriations from the Rumford fund of the American Academy and from the Bache fund of the National Academy had enabled Professor Pickering at the Harvard College Observatory to try a novel photographic method. The prism was placed in front of the object glass of the astronomical telescope, a plan suggested by Fraunhofer in 1823 for eye observations. By this arrangement not only was a great increase of light secured, but all the stars at the time in the field of the telescope impressed their spectra simultaneously upon the plate. Formerly only one star spectrum could be photographed at a time, and this only when the star was of the first or second mag-

nitude. But by the new method more than three hundred spectra have been simultaneously obtained on a single plate, some of them of stars not brighter than the seventh or eighth magnitude. The first spectra thus photographed were taken with a prism of 30° placed in front of a photographic lens of two inches aperture and of seven inches focal length. The necessary width of the spectrum was secured by the trailing of the star, no clock-work being used; and yet with this imperfect apparatus the spectra of α Lyrae and α Aquilæ showed their characteristic lines, and in the spectrum of Polaris more than a dozen lines could be counted. Subsequently a prism of eight inches clear aperture, whose refracting angle was 15° , was placed in front of a photographic lens of the same aperture, having a focal length of forty-five inches. When the photographic lens was in the meridian the refracting edge of the prism was made to be horizontal, so that the spectrum extended north and south. By accelerating or retarding the clock-work, therefore, the trailing of the star could be made to give width to the spectrum, a gain or loss of twelve seconds an hour being the rate best suited to the faintest stars.

Early in 1886 Mrs. Draper, who, from the first had taken great interest in this work, so directly a continuation of that begun by Dr. Draper with so much promise, generously offered to place at Professor Pickering's disposal the excellent eleven-inch photographic telescope which Dr. Draper had himself used so successfully in his spectrum researches, together with a sufficient sum of money to enable the new method to be put to the test on a still larger scale. In consequence Professor Pickering decided to continue the investigation along three more or less independent lines: First, he purposed making a general survey of stellar spectra, each spectrum being photographed with an exposure of not less than five minutes; second, he desired to undertake a determination of the spectra of the faintest stars, each photograph of this set receiving an exposure of an hour; and, third, he decided to carry on a careful study of the spectra of the brighter stars. The entire research was to be designated as "The Henry Draper Memorial."

In his first annual report Professor Pickering has given the results obtained during the year 1886. In the first two of the directions above mentioned the work was done with the Bache photographic telescope, 15,729 stellar spectra having been photographed and measured. The photographs taken with an exposure of five

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minutes exhibit in general the spectra of all stars brighter than the sixth magnitude with sufficient distinctness for measurement. Those taken with an hour's exposure show upon a single plate the spectra of stars included in a region ten degrees square not fainter than the ninth magnitude. For the work of studying the spectra of the brighter stars the Draper eleven-inch corrected refractor was used, four prisms being placed in front of the object-glass, each of which had a refracting angle of 15° . The original negatives thus taken were enlarged by an ingenious process. A cylindrical lens was placed close to the enlarging lens, with its axis parallel to the length of the spectrum. In the apparatus actually employed the length of the spectrum was increased five times, while its breadth was increased nearly one hundred times, so that in the plate accompanying the report a spectrum of β Geminorum is shown twenty-two centimeters long and seven and a half wide. The photographs thus enlarged are remarkable. The spectrum of α Cygni shows the H line to be double, its two components having a difference of wave length of about one ten-millionth of a millimeter; that of α Ceti shows the lines G and h as bright lines, as also the four ultra-violet lines characteristic of spectra of the first type, to which Dr. Huggins gave the letters α , β , γ , and δ . But the lines H and K are dark, showing that they do not belong to the same series. The spectrum of α Tauri shows a multitude of lines and bands massed in the more refrangible region; that of Sirius shows a large number of faint lines in addition to the well-known and characteristic broad bands; that of α Canis Minoris, taken with four prisms, shows the solar lines G, h, H, and K.

So well satisfied was Mrs. Draper with these splendid results that she decided to extend greatly the original plan of the work and to include all departments of the subject, so that the final results shall form a complete discussion of the constitution and conditions of the stars as revealed by their spectra, so far as scientific methods at present permit. The investigations now in progress under Professor Pickering's direction comprise (1) a catalogue of the spectra of all stars north of -24° of the sixth magnitude or brighter; (2) a more extensive catalogue of spectra of stars brighter than the eighth magnitude, and (3) a detailed study of the spectra of the bright stars, including a classification of the spectra, a determination of the wave lengths of the lines, a comparison with terrestrial spectra, and an application of the results to the measure-

ment of the approach and recession of the stars. For these purposes the two reflecting telescopes constructed by Dr. Draper will be used, in addition to the fifteen-inch refractor of the Harvard College Observatory and the instruments hitherto employed. "But," says Professor Pickering, "Mrs. Draper has provided not only the means for keeping these instruments actively employed, several of them during the whole of every clear night, but also the means of reducing the results by a considerable force of computers and of publishing them in a suitable form. A field of work of great extent and promise is open, and there seems to be an opportunity to erect to the name of Dr. Henry Draper a memorial such as heretofore no astronomer has received."

While using the gelatino-bromide dry process in stellar-spectrum photography, Dr. Draper conceived that the great sensitiveness of these plates might enable him to secure a photograph of a nebula, and in this way to obtain an accurate record of its present condition with a view to future comparisons. On September 30th, 1880, after an exposure of fifty-seven minutes, he obtained a photograph of the great nebula in Orion, which was sufficiently perfect to enlarge. In order to get the much fainter portions of the nebula, however, the plates had to be overexposed for the stars within it. Satisfied that the project was entirely feasible, he devoted a considerable time to securing the greatest possible perfection in the driving clock and to improving the details of the photographic manipulation. In March, 1881, a second and much better photograph of this nebula was taken, with an exposure of 104 minutes. Finally, a year later, on the 14th of March, 1882, he succeeded in making a successful exposure of 137 minutes, and in producing a superb photograph, which showed stars of the 14.7 magnitude of Pogson's scale invisible to the eye, and in which the faint outlying regions of the nebula itself were clearly and beautifully shown. This unrivaled photograph, by far the most brilliant success achieved by celestial photography up to that time, will ever have a high astronomical value, since by comparing with it photographs of this nebula taken many years subsequently, any changes which are taking place in its constitution may be traced and their history written. Ordinarily a photograph of the spectrum of an object is more difficult to obtain than a photograph of the object itself; but in the case of a nebula this is not true. The spectrum in this case being of bright lines, its light is localized and readily impresses the plate. Moreover, any error in the rate of the clock

or any tremor of the instrument, which in the case of the nebula itself would be fatal to the definition, counts for little in photographing its spectrum; since the image being simply shifted off the slit thereby produces no injurious results upon the definition. Many excellent photographs of the spectrum of the nebula in Orion were obtained by Dr. Draper in addition to those of the nebula itself. Two contrivances were made use of to obtain this spectrum: First, a direct-vision prism placed in the cone of rays from the objective before they had reached a focus; and, second, the two-prism spectroscope which had been employed to produce stellar spectra. To obtain good results with the direct-vision prism without a slit, it was necessary that the image should be kept stationary on the sensitive plate throughout the entire exposure of two hours. The most striking feature of these spectrum photographs, perhaps, is the continuous spectrum of the two portions of the nebula just preceding the trapezium, indicating that at these points there is condensed matter, either in the form of gas under great pressure or in the liquid or solid form. But neither in these portions nor in others which show the continuous spectrum more faintly was Dr. Draper able to detect any stars of a magnitude sufficient to produce this effect.

Of these spectra Dr. Draper writes as follows: "The hydrogen line near G, wave length 4,340, is strong and sharply defined; that at *h*, wave length 4,101, is more delicate, and there are faint traces of other lines in the violet. Among these lines there is one point of difference, especially well shown in a photograph where the slit was placed in a north-and-south direction across the trapezium; the H γ line, λ 4,340, is of the same length as the slit, and where it intersects the spectrum of the trapezium stars a duplication of effect is visible. If this is not due to flickering motion in the atmosphere it would indicate that hydrogen gas was present even between the eye and the trapezium. I think the same is true of the H δ line, λ 4,101. But in the case of two other faint lines in this vicinity I think the lines are not of the length of the slit, one being quite short and the other discontinuous. If this observation should be confirmed by future photographs of greater strength it might point to a non-homogeneous constitution of the nebula, though differences of intrinsic brightness would require to be eliminated."

"As illustrating the delicacy of working required in this research it may be mentioned that in one of these photographs the spectrum of a star of the tenth magnitude is easily discerned. It is only a

short time since it was considered a feat to get the image of a ninth-magnitude star, and now the light of a star of one magnitude less may be photographed even when dispersed into a spectrum."

On the 14th of May, 1880, Dr. Draper communicated to the Royal Astronomical Society the results of some observations upon the spectrum of Jupiter, going to show that this planet emitted intrinsic light. Generally the close resemblance between the spectrum of Jupiter and that of the sun made it quite certain that most, if not all, of the light of the planet was simply reflected sunlight—a fact which enabled Dr. Draper to use its spectrum for reference in stellar work. But on the 27th of September, 1879, a spectrum photograph was obtained showing a modified condition of things. The image of the planet was thrown upon the slit of the spectroscope by a telescope of 183 inches focal length, this slit being placed approximately in the direction of a line joining the poles, so that the spectrum was that of a band extending across the disk at right angles to the equator. The exposure required was fifty minutes. The peculiarities observed in the photograph are not due to any change in the number or the position of the Fraunhofer lines, but to a variation in the strength of the background. In the case of the moon-spectrum, photographed for comparison on the same plate with an exposure of ten minutes, the background in any region is uniform across the width of the spectrum; while in the spectrum of Jupiter the background is fainter in the middle of the width of the spectrum in the region above the line *h*, and stronger in the middle in the region below *h*, especially toward F. These modifications in the spectrum seemed to Dr. Draper to indicate, first, an absorption of solar light in the equatorial regions of the planet, and, second, a production of intrinsic light at the same place. To reconcile these apparently opposite indications, he suggested the hypothesis that the temperature of the incandescent substances producing light at the equatorial regions of Jupiter did not suffice for the emission of the more refrangible rays, and that there were present materials which absorbed those rays from the sunlight falling on the planet. While, therefore, the absorption phenomena are not of special interest, the strengthening of the spectrum between *h* and F in the portions answering to the vicinity of the equatorial regions of Jupiter seems to bear very directly on the problem of the physical condition of this planet as to incandescence, and to be consequently of very considerable significance.

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The large comet of 1881 was made a subject of investigation by Dr. Draper. On the 24th of June a photograph of the nucleus and part of the envelopes was obtained in 17 minutes; and subsequently, by an exposure of 162 minutes, a photograph was secured showing the tail ten degrees in length. He next attempted, by placing a direct-vision prism between the sensitive plate and the object-glass, to obtain a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. An exposure of 83 minutes gave a strong picture of the spectrum of the nucleus, coma, and part of the tail, but the banded spectrum was overpowered by the continuous spectrum. The two-prism stellar spectroscope was then used, in order to weaken the continuous spectrum by the increased dispersion, and with this arrangement three photographs of the cometary spectrum were taken, the exposures being 180, 196, and 228 minutes, respectively, a comparison spectrum being taken with each. The most striking feature of these photographs is a heavy band above H, which is divisible into lines, and two faint bands, one between G and *h* and another between *h* and H; thus strengthening the hypothesis of the presence of carbon in comets.

The work at the Hastings Observatory was carried on, for the most part, during the summer months, Dr. Draper then residing at his country place in Dobbs Ferry, two miles distant. His investigations during the winter were made at his house in New York city, those researches being selected for the purpose which did not require the use of the telescope. At first two rooms upon the third floor were devoted to these winter investigations, and here all his earlier work, such as the construction of the 28-inch mirror and the production of the diffraction spectrum photographs was done. Subsequently, however, needing additional room and additional facilities, he built a special physical laboratory as the third story of his stable, situated directly in the rear of his dwelling, this laboratory being connected with the house by a covered way. The equipment of this laboratory is superb. A siderostat, by Alvan Clark & Sons, placed upon the roof, furnishes abundant sunlight, directed to any part of the room by a secondary mirror. An Otto gas engine of four-horse power gives motion to three dynamo machines for the production of electric currents of different intensities. One of these is a Gramme machine, the armature of which is wound double, with a collector on each side. In consequence, not only may the two sides be used independently or connected together in series or

in multiple, but by an ingenious modification constructed by Dr. Draper they may be made to give continuous or alternating currents at will. The second dynamo is an Edison shunt machine, used mainly to light the laboratory by means of incandescent lamps. The third is a Maxim dynamo, the current of which is used to maintain arc lights for illuminating or experimental purposes, and also to maintain the field of the Gramme when run as an alternating machine. For the production of the electric spark an induction coil of large size is employed, made by Ruhmkorff. When used with the direct current it gives 18-inch sparks readily, although in order to avoid perforation the safety points are usually set at 10 or 12 inches. By means of an ingenious interrupter, attached to the axis of the Gramme machine, this coil, used with the direct current, can be made to give 1,000 10-inch sparks per minute. With the alternating current, which requires, of course, no interrupter, the spark, although silent and only about one-quarter as long, is of much greater volume; so that, with a condenser of considerable capacity in the secondary circuit, the discharge is like the rattle of musketry. The optical and photographic appliances of this laboratory are of the finest. Not only are there complete spectroscopes and cameras of all sorts, but there are also lenses, prisms, and gratings of the best materials and the finest workmanship, ready to be used in extemporizing any apparatus needed in research. A lathe, a file bench, and a carpenter's bench, each with its full set of tools, and a conveniently arranged dark room for photographic work, complete the appointments of this beautifully finished room.

It was in his New York laboratory that Dr. Draper made his important research upon the presence of oxygen in the sun, his first paper upon this subject having been presented to the American Philosophical Society in April, 1877. Already, in the year 1869, he had begun an investigation upon the photographic spectra of the elements, particularly of the metalloids carbon, nitrogen, and hydrogen, as preliminary to a comparison of these spectra with those of the sun and stars, especially in the more refrangible regions. The first photographs of metallic spectra were taken with apparatus at hand in the laboratory, a couple of Bunsen batteries being used to work an induction coil giving a half-inch spark, and a Hoffman direct-vision spectroscope producing the spectrum, which was less than an inch in length from G to H. This apparatus was soon replaced by a larger battery, a two-inch induction coil, and a Brown-

ing direct-vision prism of an inch aperture. Then the size of the coil was still farther increased, first to one giving six-inch sparks and then to a large Ruhmkorff machine capable of giving a spark of 18 inches. The battery was ultimately replaced by a Gramme dynamo machine, purchased at the Centennial Exhibition in 1876, and which was capable of giving an arc light of 500 candles. When used with the large coil it gave readily a thousand ten-inch sparks per minute; but this spark, even when condensed with fourteen Leyden jars, each having half a square foot of coated surface, placed in the secondary circuit, although of intense brilliancy, gave by measurement a light of only about one candle. The connections of the dynamo machine were so arranged that one-half of the armature gave a continuous current through the field magnets, while the current from the other half was used in the external circuit.

At first a Foucault mercurial interrupter was arranged to make and break the current passing into the primary circuit of the induction coil. But when the current was necessarily increased up to that given by the machine when making a thousand revolutions a minute, the mercury was driven violently out of the cup whenever the interrupter was in action. Hence it became essential to arrange a mechanical break, using solid metal only, a requirement met by Dr. Draper by the use upon the axis of the armature of a wheel with an interrupted rim. The arrangement of the terminals so as to get the steadiest and brightest effect offered great difficulties. The condensed spark taken in the open air or in a gas under atmospheric pressure pursues, if unconfined, a zigzag course, which tends to widen the lines in the photographed spectrum. After many experiments, however, it turned out that the spark could be compressed between two plates of thick glass; or, better yet, between two plates of soapstone; so that, under these conditions, if the interval between the plates was directed towards the slit of the spectroscope, the lateral flickering of the spark was entirely prevented, while at the same time the spark itself was freely exposed to the slit, and this without the intervention of glass or any substance on which the volatilized metal from the terminals could form a deposit. Early in the research it became apparent that Plücker's tubes could not be employed with electrical currents of more than a certain intensity, partly on account of the deposit that took place in the capillary portion and partly because the terminals became so hot as to melt and crack the glass.

The optical part of the apparatus underwent many modifications in the course of the research. The Hoffman direct-vision prism, combined with a lens of six inches focus, at first used, was soon replaced by a Browning direct-vision prism and a lens of eighteen inches focus, the latter being arranged for conjugate foci, so that it was virtually as if collimating and observing lenses of thirty-six inches focus were employed. The final system consisted of a collimator of two inches aperture and twenty-six inches focus, succeeded by two bisulphide of carbon prisms of two inches aperture and an observing or photographing lens of six and a half feet focal length. This system gave a dispersion of about eight inches between G and H, so that original negatives could be obtained with it on a scale about half the size of Angström's charts.

In order to facilitate accurate comparison between elemental spectra and the spectrum of the sun or a star, Dr. Draper arranged his apparatus so that the two spectra to be compared could be photographed closely superposed upon the same plate. The slit of the spectroscope was about an inch long, and opposite its lower half was placed a right-angled prism, which reflected through this half the sunlight from a heliostat. The spark-compressor was placed opposite the upper half of the slit, which it illuminated directly. In this way the solar spectrum and the air spectrum were thrown upon the plate by the same optical train. The terminals used were of iron and of aluminum, the object being to show by the coincidence of the bright iron lines with the corresponding dark iron lines of the sun spectrum that no shifting had taken place in the interval of changing from one source of light to the other.

Up to 1876 the photographs taken were on so small a scale that they did not give rise even to a suspicion of the presence of oxygen in the sun. During this year, however, original negatives two inches long from G to H were secured, which bore an enlargement of three or four times quite well, and which seemed to prove the coincidence between the oxygen and the solar lines very satisfactorily. An Albertype print of such an enlargement accompanied Dr. Draper's original paper of 1877. Of this plate Dr. Draper says: "No close observation is needed to demonstrate to even the most casual observer that the oxygen lines are found in the sun as bright lines, while the iron lines have dark representatives. The bright iron line at G (4,307), on account of the intentional overlapping of the two spectra, can be seen passing up into the dark absorp-

tion line in the sun. At the same time the quadruple oxygen line between 4,345 and 4,350 coincides exactly with the bright group in the solar spectrum above. This oxygen group alone is almost sufficient to prove the presence of oxygen in the sun, for not only does each of the four components have a representative in the solar spectrum, but the relative strength and the general aspect of the lines in each case is similar. The fine double line at 4,319, 4,317 is plainly represented in the sun. Again, there is a remarkable coincidence in the double line at 4,190, 4,184. The line at 4,133 is very distinctly marked. The strongest oxygen line is the triple one at 4,076, 4,072, 4,069; and here, again, a fine coincidence is seen, though the air spectrum seems proportionately stronger than the solar."

In consequence of these coincidences Dr. Draper concludes that "oxygen discloses itself by bright lines or bands in the solar spectrum and does not give dark absorption lines like the metals. We must, therefore, change our theory of the solar spectrum and no longer regard it merely as a continuous spectrum with certain rays absorbed by a layer of ignited metallic vapors, but as having also bright lines and bands superposed on the background of continuous spectrum. Such a conception not only opens the way to the discovery of others of the non-metals, sulphur, phosphorus, selenium, chlorine, bromine, iodine, fluorine, carbon, etc., but may also account for some of the so-called dark lines by regarding them as intervals between bright lines. The bright lines of oxygen in the spectrum of the solar disk have not been hitherto perceived probably from the fact that in eye observations bright lines on a less bright background do not make the impression on the mind that dark lines do. When attention is called to their presence they are readily enough seen even without the aid of a reference spectrum. The photograph, however, brings them into greater prominence. From purely theoretical considerations derived from terrestrial chemistry and the nebula hypothesis the presence of oxygen in the sun might have been strongly suspected, for this element is currently stated to form eight-ninths of the water of the globe, one-third of the crust of the earth, and one-fifth of the air, and should, therefore, probably be a large constituent of every member of the solar system. At first sight it seems rather difficult to believe that an ignited gas in the solar envelope should not be indicated by dark lines in the solar spectrum and should appear not to act under the law 'a gas when

ignited absorbs rays of the same refrangibility as those which it emits.' But in fact the substances hitherto investigated in the sun are really metallic vapors, hydrogen probably coming under that rule. The non-metals obviously may behave differently. It is easy to speculate on the causes of such behavior, and it may be suggested that the reason of the non-appearance of a dark line may be that the intensity of the light from a great thickness of ignited oxygen overpowers the effect of the photosphere just as if a person were to look at a candle flame through a yard thickness of ignited sodium vapor he would see bright sodium lines only and no dark absorption lines."

The second paper on the presence of oxygen in the sun was read by Dr. Draper before the Royal Astronomical Society on the 19th of June, 1879. In order to meet the criticism that perhaps the dispersion hitherto employed was not sufficient to disclose the lack of coincidence, if it really existed, this paper was accompanied by an Albortype print showing the juxtaposed spectra of air and the sun on a scale about twice the size of Angström's chart, the negative thus enlarged having been taken with a dispersion about four times that formerly used. In speaking of this plate Dr. Draper says: "Of course an enlargement never does justice to the original from which it was produced, and, in order to study the matter faithfully, the negative must be examined carefully with a magnifier. Besides this, owing partly to the fact that the solar spectrum has suffered from absorptive influences, both in the earth's atmosphere and in the solar atmosphere, the conditions under which the oxygen spectrum is seen when compared with the spark spectra are modified. In fact, a critical study of the two spectra demands that each line of oxygen should be separately photographed with the corresponding region of the sun's spectrum, so as to reproduce as nearly as possible the same conditions for each. As an instance of the modifications which may be caused by the solar atmosphere the superposition of absorption lines on the bright lines of oxygen may be mentioned. If, as seems to be the case, the stratum giving the oxygen spectrum in the sun lies deeper than the reversing layer in which iron exists, I see no reason why an iron absorption line, for instance, may not fall upon an oxygen bright band. If it be conceded that there are bright lines in the spectrum of the solar disk, which seems to be the opinion of several physicists, and especially Lockyer, Cornu, and Hennessy, the question of their origin naturally attracts attention.

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It seems that there is great probability, from general chemical reasons, that a number of the non-metals may exist in the sun. The obvious continuation of this research is in that direction. But the subject is surrounded by exceedingly great obstacles arising principally from the difficulty of matching the conditions as to temperature, pressure, etc., found in the sun. Any one who has studied nitrogen, sulphur, or carbon, and has observed the manner in which the spectrum changes by variations of heat and pressure, will realize that it is well nigh impossible to hit upon the exact conditions under which such bodies exist at the level of the photosphere. The fact that oxygen within a certain range of variation suffers less change than others of the non-metals has been the secret of its detection in the sun. It appears to have a greater stability of constitution, though Schuster has shown that its spectrum may be made to vary. I have already begun an extended series of experiments on the non-metals, but the results exhibit such confusion that their bearing cannot at present be distinctly seen. In the case of nitrogen the broad bands between G and H exhibit under the most intense incandescence a tendency to condense into narrow bands or lines, and indeed there are some sharp lines of nitrogen in the photograph now presented. It does not follow, therefore, that the bright bands of oxygen are necessarily the brightest parts of the solar spectrum. Other substances may produce lines or bands of greater brilliancy.

"There is also another cause for a difference of appearance in a bright-line spectrum produced in a laboratory and bright lines in the sun. While the edges of a band in the spark spectrum may be nebulous or shaded off, the corresponding band in the solar spectrum may have its edges sharpened by the action of adjacent dark lines due to one or another of the metallic substances in the sun."

"On the whole, it does not seem improper for me to take the ground that, having shown by photographs that the bright lines of the oxygen spark spectrum all fall opposite bright portions of the solar spectrum, I have established the probability of the existence of oxygen in the sun. Causes that can modify in some measure the character of the bright bands of the solar spectrum obviously exist in the sun, and these, it may be inferred, exert influence enough to account for such minor differences as may be detected."

An interesting discussion followed the reading of this paper. Referring to the earlier and the later photographs, Mr. Ranyard said:

"If Dr. Draper has increased his dispersion four times he has not merely increased the probability of his case four times, but he has increased the value of every coincidence he shows four times. On looking at the original photographs (which show the coincidences more sharply than the paper prints) I counted eighteen oxygen lines, and therefore the increase of probability on the present occasion as compared with the former occasion is as four to the eighteenth power to one, a very enormous number. There are two or three ways of looking at the probability of the proposition which Dr. Draper has laid down, that the bright lines of oxygen coincide with the bright lines in the solar spectrum. In the first place there is the chance that the center of no single line of oxygen should fall opposite to a dark line or space in the solar spectrum. Let us suppose that the breadth occupied by dark lines in the solar spectrum is equal to the breadth occupied by bright lines (probably, as seen with high dispersion, this is well within the truth), then the chance of any oxygen line falling opposite to a bright line or interspace in the solar spectrum would be one-half; and if the eighteen lines of oxygen had been thrown down at random beside the solar spectrum the chance that the center of all the lines should fall opposite to bright spaces would be one divided by two to the eighteenth power." Mr. Christie, while expressing his great admiration at the splendid results which Dr. Draper had achieved, was afraid that the question which he had undertaken was a very difficult one. "According to Dr. Draper's supposition," he said, "the solar spectrum is made up of a continuous spectrum with dark absorption lines and also bright lines superposed upon it. Now, the ordinary spectrum, which up to the present time we have supposed that we had to deal with, was a continuous spectrum with dark lines; but when you superpose on this bright lines so faint that you cannot distinguish their brightness from the general background of the spectrum, it is evident that the problem becomes more complicated." Mr. Proctor regarded as of great importance the fact that "all the oxygen lines fall entirely opposite to bright interspaces in the solar spectrum and that none of them even partially overlap dark lines." He asked whether it was "antecedently probable that there should be any continuous background in the solar spectrum. If the photosphere is purely gaseous we should have only bright bands interfered with and modified by absorption lines." Dr. Gladstone expressed the opinion that Dr. Draper's results had "largely increased the evi-

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dence of there being real coincidences between the oxygen lines and bright spaces in the solar spectrum." "There seems to be still a great question," he said, "as to whether the solar spectrum is made up only of bright and dark lines or whether there is a background of continuous spectrum. I am not disposed to give up the idea that we have a continuous spectrum underlying these dark lines, but think that it is certain that we have also bright lines mixed with the dark. We know that when we look at the edge of the sun there are bright lines corresponding to hydrogen and some other elements to be seen, but there are no oxygen lines. Now, I would suggest that this shows that the oxygen never rises to the level of the chromosphere, so as to be seen at the limb of the sun, and probably that is just the reason why we see its lines as bright lines and not as dark lines, for it never gets up to a level where it is sufficiently cool to form dark lines." Dr. Huggins was "overwhelmed with a sense of the large amount of conscientious labor and care which Dr. Draper had evidently bestowed upon the investigation." He thought that "Dr. Draper had made out a *prima facie* case, which entitled him to demand a careful examination of the evidence he had brought forward; but for his own part he should like to suspend his judgment until he had had an opportunity to re-examine that part of the spectrum." Captain Noble considered that, looking at the photographs impartially, if we were "to deny the evidence supplied by some of these coincidences, and notably by this group of four lines, and accept Mr. Christie's dicta, we literally should have no tangible evidence as to the existence of any element in the sun at all."

The presentation of these photographic enlargements of the superposed solar and air spectra to the French Academy was made by M. Cornu on the 23d of June, 1879. Upon this occasion M. Faye, after alluding to the remarkable fact that up to the present time spectrum analysis had furnished no evidence whatever of the presence of oxygen in the sun, went on to say: "M. Draper, however, has succeeded in discovering the oxygen not in the chromosphere but in the photosphere, where it discloses itself by bright lines. It is obvious that if this gas is dissociated at a depth, it is immediately taken up by multiple combinations in the region of and at the temperature of the brilliant surface. I see in these facts the hope of a confirmation and, above all, of an extension of the views I have put forth on the constitution of the sun; but, whatever may be the fate that the

progress of spectrum analysis reserves to them, I express here my admiration for the discovery of M. Draper, and I hope that his results, so well confirmed by the photographic proofs that our learned member, M. Cornu, has shown to the Academy, will meet with no delay in being universally accepted by competent judges."

During the past year a paper upon the presence of oxygen in the sun has been published by Professor Trowbridge and Mr. Hutchins. Referring to the difficulties encountered by Dr. Draper, these authors say: "The time that has elapsed since his work does not seem to have made those difficulties less, and in spite of all that our ingenuity has been able to devise, we have been practically confined to taking the spark in free air or oxygen at atmospheric pressure, notwithstanding the broad and hazy character of the lines under these conditions." The electrical portion of their apparatus was essentially similar to Dr. Draper's. An alternating dynamo, whose current was sharply interrupted, was used to excite three large quantity induction coils connected in series, from two to twelve jars being placed in the secondary circuit. The spark discharge, taken between two aluminum rods placed in front of the slit, although producing a deafening rattle, gave a light of only about two candles. For producing the spectrum these authors used a Rowland concave grating having a ruled surface six inches by two. A quartz lens of five feet focus produced upon the slit an image of the sun reflected from a heliostat. The two spectra upon the negative lie closely edge to edge, so that accurate comparison is readily made between them. The wave lengths of the air lines were obtained by comparison with Rowland's map of the sun spectrum and a list is given of the wave lengths of nearly three hundred lines thus photographed in the air spectrum between wave lengths 3,749 and 5,034.

It is interesting to notice the close agreement of Dr. Draper's wave lengths as given by him for the oxygen lines with those obtained by means of this greatly improved apparatus. Dr. Draper speaks of a triple oxygen line between 4,345 and 4,350. The above list mentions four strong lines in the air spectrum at 4,347.94, 4,349.30, 4,351.40, and 4,353.70, and of a fifth at 4,345.52. To the Draper double line at 4,319, 4,317 correspond in the list two strong lines 4,319.50 and 4,317.20. The double line 4,190, 4,184 is given in the list as 4,190, 4,185.32, both very strong. Draper's line at 4,133 is given as 4,132.82, faint. The triple line, 4,076, 4,072, and 4,069, which Draper gives as the strongest line of oxygen,

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is given as 4,076.19, 4,072.34, 4,070.24, all strong. Besides these eleven oxygen lines, Draper's large photograph shows five others of wave lengths approximately 4,155, 4,147, 4,119, 4,105, and 4,093, making the sixteen lines identified as oxygen lines, given in this photograph. As this plate, which is about twenty inches long, extends only from wave length 4,025 to 4,375, the total number of lines in the Trowbridge and Hutchins list corresponding to this range is ninety. Of these, eighteen only are marked strong in the list; and of these eighteen, thirteen apparently are shown on Dr. Draper's photograph; the remaining three of Dr. Draper's lines—i. e., 4,155, 4,147, and 4,133—being faint. Of the five list lines remaining, that at 4,109 may be a member of the 4,105 group, 4,349 may belong with 4,347, and 4,353 with 4,351; 4,280 does not seem to have been noticed by Dr. Draper, and 4,367 is near the edge of his plate. Possibly, too, some of them are nitrogen lines.

Of the eighteen lines prominent in that portion of the spectrum photographed by Dr. Draper, only three are mentioned in the list as agreeing; and of these, the words "may agree" indicate only a possible agreement. It is not stated in the paper itself what agreement means exactly. Presumably it means that the line opposite to the word "agrees" coincides with a dark line in the solar spectrum. If this be so, then it would appear that, since none of the sixteen lines observed by Dr. Draper and given in the list agree with dark solar lines, they must agree with the interspaces, as Dr. Draper himself believed. Applying the same rule to the entire spectrum photographed by Professor Trowbridge and Mr. Hutchins, it appears that out of the 288 air lines whose wave lengths are given only eighteen are stated to agree with lines in the solar spectrum, while eighteen others may possibly agree. These agreements may perhaps be due to metallic vapors, since the spark taken between aluminum terminals is stated to have been composite. If any fact is clearly established by this research, therefore, it would seem to be the fact that the lines in the spectrum of air, taken under the conditions stated, do not agree with the dark lines of the sun spectrum; and hence that oxygen—and, perhaps, nitrogen also—is not contained, under the conditions used in these experiments, in the chromosphere or reversing solar layer. But since, with the length of spectrum used, it would not seem probable that the air lines would extend over the dark lines and the interspaces also, the conclusion seems natural that if the oxygen lines do not coincide with

dark lines they must coincide with bright ones, or at least must agree with the interspaces between the dark lines.

Moreover, in the above paper the authors remark: "The bright bands of Dr. H. Draper's spectrum are found to be occupied by numerous dark lines of various degrees of intensity." But this, as Dr. Draper himself has shown, does not in the least invalidate his theory. In the discussion before the Royal Astronomical Society Mr. Ranyard also pointed out this possibility. He said: "The observations of the bright lines seen at the limb of the sun renders it probable, that the layer of the solar atmosphere which gives rise to the dark lines lies above the layer from which we receive the light of the bright oxygen spectrum, for no bright oxygen lines are seen in the chromosphere. So that it is possible to conceive that the oxygen lines of the solar spectrum may be modified as we observe them by the superposition of dark lines, and this appears in one or two instances to have been the case; but the character of the bright interspaces is very little changed." * * * "With regard to the bright lines falling opposite interspaces which are broader than the bright lines of oxygen, the probability is very great against there being two adjacent bright lines of exactly equal brightness; but it must be remembered that we are not examining the bright lines themselves, but only photographs of the lines, and that the bright parts of these photographs are what would be called by photographers over-exposed; consequently the gradations of brightness are very much obliterated." Indeed the authors themselves say that "it happens quite frequently that an oxygen line falls centrally upon the space between two dark lines of the solar spectrum." But they add that this takes place "not more frequently than we might expect as a matter of chance."

It would appear, therefore, that the question of the existence of oxygen in the sun resolves itself finally into the existence in the solar spectrum of bright bands. Reasoning *a priori*, there is not only no reason for their non-existence there, but there is good reason for their presence; since it depends solely upon the temperature and the radiating power of the oxygen under the conditions in which it exists on the sun's surface whether the lines it gives are brighter or darker than the general surface, as Meldola has shown. Moreover, as a matter of fact, bright lines are common in the spectra of many stars, and many astronomers maintain their existence even in the solar spectrum. Obviously they must be much better seen with low than

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with high dispersion; and some of the interspaces in Dr. Draper's large photograph produce strongly upon the mind the impression of bright bands. Moreover, upon what absolute proof does the background of continuous spectrum rest? Is not Mr. Proctor's suggestion that the photosphere is purely gaseous quite as probable as any other? And if so, then the solar spectrum may consist only of bright bands interfered with and modified by absorption bands.

The final solution of this question must await further light upon the constitution of the spectrum of the sun. The careful research made at the Jefferson Laboratory seems to have proven conclusively that the lines of oxygen obtained under the given conditions do not coincide with the dark lines of the solar spectrum, and therefore to have proved that these lines do coincide with the interspaces. If these interspaces are simply gaps between the dark lines through which the background of photospheric continuous spectrum appears, then the coincidence of the oxygen lines with these interspaces must be accepted as evidence, by the same reasoning that is applied to metallic lines, that oxygen is not present in the sun—at least in the same condition as that in which we know it. But if the solar spectrum does contain actual bright lines or bands superposed like the dark lines, upon a continuous spectrum; or much more, if this spectrum is made up only of bright and dark bands or lines, certainly a possible supposition, then it would seem clear that the coincidence of the bright oxygen lines with these bright solar lines is proof of the existence of oxygen in the sun of the same character as the proof of the existence of iron there, the increased radiation producing this reversal in intensity. But this is exactly Dr. Draper's theory, and so far, at least, it does not seem to have been disproved.

The new physical laboratory was completed in January, 1880. Dr. Draper's chief object in building it, as we have seen, was to secure increased facilities for his investigations upon the spectra of the elements, particularly those of the non-metals. The photographic work for the oxygen research had been done in a back room of the third story of his New York residence. The prisms which produced the spectra were hollow prisms filled with carbon disulphide. They were loaned for this purpose by Mr. Rutherford and had been used by him originally in obtaining his celebrated photograph of the solar prismatic spectrum. The temperature of this back room proved to be remarkably uniform, so that the quality of the photographs taken was excellent and their sharpness all that

could be desired. When, however, the preliminary experiments required for the continuation and extension of this research were undertaken in the new physical laboratory, it was found practically impossible to use carbon disulphide prisms in this room, owing to the rapid variations of temperature which took place there. No definition whatever could be obtained with the same prisms which had performed so well in the main house. In consequence, the use of these prisms was abandoned, and a series of experiments was undertaken to obtain the spectrum by other means. First a Rutherford silvered glass grating of 8,640 lines to the inch was employed, and then a train of six Steinheil flint-glass prisms. With reference to these experiments, Dr. Draper says: "The exposures required when using the silvered grating were so long that experimentation was very tedious; but when in addition the definition did not equal that of the two bisulphide prisms formerly used a change became necessary. If we could overcome the effect of temperature on bisulphide it would doubtless serve our purpose best, because it is more transparent, less colored, and loses less light by reflection than glass prisms, since there are only two prisms needed to do the work of four flints. But the instability of a bisulphide train is so marked in the new laboratory, on account of the fluctuations of temperature, that we have not been able to depend upon it. Possibly if the prisms were enclosed in cotton batting or immersed in water these difficulties might be overcome." These experiments extended through nearly two years, beginning in March, 1880 and continuing until the early winter of 1881-'2.

Among the experiments undertaken in the new laboratory was a series made to test the performance of a disulphide prism of Thollon's construction, made by Hilger, of London, and obtained by Dr. Draper when in Europe, in the spring of 1879. This prism consists of a glass bottle ground away so as to have two plane sides, including between them an angle of 90° . Upon these sides are cemented two prisms of flint glass four inches by two upon the face, each having a refracting angle of 18° , the refracting edges of these flint prisms being opposed to that of the disulphide prism within. The compound prism thus made has a refracting angle of 54° . In using it the same difficulties were encountered which had been experienced with the Rutherford disulphide prism. Owing to the temperature variations, the spectrum lines were "wooly" and no definition was possible. Dr. Draper says: "The bisulphide prism touched with

the finger or breathed on loses all definition at once." The result, however, was sufficient to show that the dispersive power of the Thollon prism was equal to that of about four of the Steinheil flint prisms, and this fact made it so extremely desirable to use this Thollon prism that Dr. Draper was led to investigate the cause of the unsteadiness of the disulphide, with a view of remedying it, if possible.

The difficulty now referred to has been encountered by all experimenters who have attempted to use carbon disulphide in prisms. Mr. Rutherford himself found it a serious one in his experience, and M. Thollon says that disulphide prisms must be protected carefully from temperature variations. While using his disulphide train Mr. Rutherford made an observation of great importance. He noticed that "if a good prism which with a high power refuses to define the soda line (a more stringent test than solar lines) is violently shaken and then placed in position it will for a few minutes define beautifully, but gradually settle into its former condition." It occurred to Dr. Draper, therefore, that possibly the striæ caused by convection currents, which were produced by inequalities of temperature and which occasioned the bad definition, might be destroyed by an active agitation of the liquid. Accordingly, on the 19th of November, 1881, he placed a small sheet-metal propeller in the disulphide within the Thollon prism, the driving shaft coming out through an opening in the stopper. By means of a small electric motor, this propeller could be rotated at any convenient speed. The result was marvelous; this simple device, by keeping the liquid in constant agitation, destroyed all inequalities of density, and the definition became excellent. On the 21st of November, with the stirrer revolving five times a second, the definition was so perfect that even with a faint flame the sodium lines could be seen distinctly reversed. A series of comparisons between the Thollon prism thus arranged and the train of Steinheil prisms showed that, while the relative dispersion was very nearly equal for the two, the amount of light transmitted by the Thollon was, on the average, four times, and in the region about G eight times as great as that obtained with the Steinheil train.

But now a new source of error was developed. Although by means of the revolving propeller the definition given by the disulphide prism was rendered independent of temperature changes, yet now it was found that these temperature changes, by altering the

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refractive indices of the liquid for the various spectrum lines, caused a gradual shifting of position in these lines, either in one direction or the other, as the temperature rose or fell. Obviously, during an exposure of any considerable duration, such as is often necessary with faint spectra, the change of position of the spectrum lines, produced by change of temperature, might be sufficient to destroy absolutely the definition of the image on the photographic plate. The amount of this displacement was quite considerable. During the night of December 8th the thermometer in the laboratory fell nine degrees Fahrenheit, and this temperature change caused a shifting of the sodium lines through a distance of 0.75 inch towards the more refrangible end of the spectrum. A direct experiment, made by placing a thermometer in the disulphide, showed that for a temperature change of 3.75° F. a displacement of 0.375 inch took place in the position of the sodium lines. In the hope of correcting this new difficulty an even-temperature box was constructed, surrounding the prism and filled with cotton. This box was 30 inches on a side, a plate of iron six inches square being let into the bottom and so arranged that it could be heated by a gas flame. An iron tube nine inches long and closed at the upper end passed through this plate and projected seven inches into the box. The temperature within was regulated by means of two compound expansion bars made of strips of vulcanite and brass riveted together, these strips being included in the circuit of an electro-magnet and a battery and so adjusted that when the ends of the bars came in contact as the temperature rose the circuit was closed and the electro-magnet acted to turn down the gas. As the temperature fell the circuit was automatically opened and the gas turned on. Subsequently a funnel was placed upon the lower end of the heating tube, and this tube was continued upward through the top of the box, an opening being made on one side of it within the box. A relay was so connected with a damper that it closed the top of this tube whenever the circuit was broken by the separation of the differential bars within the box, and opened it when the circuit was again closed. This relay acted in conjunction with the electro-magnet above mentioned, closing the tube at the top when the gas was turned on and opening it when it was turned off. When the gas-jet beneath the funnel was lighted the heated air rose into the tube and entered the box through the lateral opening in this tube. As soon as the temperature was reached for which the bars were set these bars

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came into contact and closed the electric circuits both of the electromagnet and the relay, thus cutting off the gas supply and at the same time opening the damper. The heated air in the box now passed out through the lateral opening in the tube until the temperature had fallen to the point for which the expansion bars had been set; then the contact was broken, the electric circuit was opened, and the cycle was complete.

Some experiments made with this apparatus in 1884 showed that it was remarkably effective. After careful adjustment, the temperature within the box was found to be maintained constant within one-tenth of a degree Centigrade for seven hours, during which time the sodium lines did not change their position by an amount equal to the distance between them. These experiments confirm the opinion of Dr. Draper that the irregular action of the carbon disulphide, which is the cause of its bad definition, is due to an inequality between the temperature of the disulphide and that of its enclosing vessel, producing convection currents in the liquid. So long as the temperature of the liquid rises and falls with that of the prism and of surrounding objects, so long the definition remains perfect, notwithstanding the temperature change. It is only when considerable changes of temperature take place suddenly that these convection currents appear and produce the striæ which destroy the definition. Since this is the condition of things in most working laboratories, the method of curing the evil completely by agitation, as in Dr. Draper's apparatus, makes the experimenter practically independent of these temperature changes. But now these temperature changes, though they cannot produce bad definition, do cause a shifting of the spectrum lines. This trouble is readily obviated by the use of the even-temperature box. Indeed, when this box has been at a constant temperature for a sufficient time, the definition is good even without the stirrer.

In 1878 Professor Draper organized an expedition to observe the total solar eclipse of July 29. The party consisted of himself as director, Mrs. Draper, Mr. T. A. Edison, President Henry Morton, and the writer. Dr. Draper's familiarity with the locality through which the track of the eclipse was to pass led him to select Rawlins, Wyoming, an important station on the Union Pacific railway, as the point of observation. The chief object of the expedition was to gain as precise an idea as possible of the nature of the corona, and

particularly to ascertain whether it was an incandescent gas shining by its own light or whether it shone by reflected sunlight.

The site chosen for the temporary observatory was in latitude $41^{\circ} 48' 50''$ N., and longitude 2h. 0m. 44s. W. of Washington, its height above the sea being 6,732 feet. While near the central line of totality, this station had the advantages of pure water, supplied from the granite of Cherokee Mountain, and of proximity to the railroad repair shops. The equipment consisted, 1st, of a Clark telescope of $5\frac{1}{4}$ inches aperture and 78 inches focal length, photographically corrected and equatorially mounted, with a spring-governor driving clock, the mounting being loaned by Professor Pickering; 2d, of a quadruple achromatic objective of 6 inches aperture and 21 inches focal length, provided with a Rutherford diffraction grating ruled on speculum metal, the ruled surface being 2 inches square; 3d, of a 4-inch achromatic telescope with a Merz direct-vision spectroscope; and, 4th, of a 4-inch Dollond achromatic, to which the Edison tasimeter was attached.

The photo-telespectroscope was arranged to obtain a photograph of the diffraction spectrum of the corona—a difficult and, in the opinion of many persons at the time, an impossible task. To attain success the lens must evidently be of large aperture and very short focal length; and the grating must be of the largest size and adjusted so as to utilize the beam of light to the best advantage. The whole apparatus must be equatorially mounted and driven by clock-work, so that the exposure may last during the entire totality. Preliminary experiment gave the apparatus its final form. The quadruple lens of 6 inches aperture and 21 inches focal length, gave an image of the sun of extreme brilliancy, though less than a quarter of an inch in diameter. Before the light from the lens reached its focus it was intercepted by the Rutherford grating set at an angle of sixty degrees. This threw the beam on one side and produced there three images—a central one of the sun itself and on either side of it a spectrum. These images were received on three separate sensitive plates. One of the spectra was about twice as long as the other and gave a photograph about two inches in length. It is clear that if the coronal light is that of an incandescent gas, ring-shaped images would be obtained upon the plate, as many in number as corresponded to the bright lines in the photographic region; while if the light be emitted from a glowing solid or be reflected from the sun itself a continuous spectrum image without rings would

be obtained. On developing the photographs after totality was over, Dr. Draper found that the spectrum photographs were continuous bands without the least trace of a ring.

The large equatorial was arranged to take a photograph of the corona itself, the exposure being the whole time of totality, or about 165 seconds. The resulting picture showed that the corona is not arranged centrally with regard to the sun. The great mass of the matter lies in the plane of the ecliptic, but is not equally distributed. To the eye it extended about a degree and a half from the sun towards the west, although it was scarcely a degree in length toward the east; while in the photograph it extended to a height of not over twenty minutes of arc, or something more than 500,000 miles. If, therefore, the corona has the structure of a meteoric mass, the mass is probably arranged in the form of an ellipsoid about the sun.

The analyzing slit spectroscope of Merz was mounted upon a telescope of four inches aperture. It was provided with two compound direct-vision prisms, of which one or both could be used at will, each consisting of five single prisms, two being of flint and three of crown glass; the former and one of the latter prisms having a refracting angle of 84° , the other an angle of 87° . This dispersive power of each of these compound prisms was about equal to that of two equilateral prisms of good flint glass. The instrument had a collimating and an observing telescope each furnished with an object-glass two-thirds of an inch in aperture and four inches in focal length. During totality, the slit being placed radially, the spectrum observed, which was that of the region near G, seemed entirely continuous; nor did any bright lines appear when the green region was examined. Upon closing the slit, however, the region from *b* to G appeared filled with dark lines on the brighter background, these lines being readily recognized as those of Fraunhofer. Of these lines *b* and F were especially distinct; D, E, and G, though easily recognized, were less prominent.

The tasimeter of Dr. Edison was attached to a four-inch Dollond telescope. In this instrument a strip of vulcanite, placed between a fixed point and a soft carbon disk or button, was arranged so that its expansion by heat produced pressure upon the carbon disk, thus decreasing its resistance and allowing more current to flow through it. The tasimeter, adjusted to three ohms, was placed in one of the arms of a Wheatstone bridge, the opposite arm having a resistance of three ohms, and the pair of arms ten ohms each. A Thomson

reflecting galvanometer was placed in the bridge wire. On the night of July 27th, says Dr. Edison in his Report, the image of the star Arcturus was brought upon the vulcanite strip. The spot of light reflected from the galvanometer moved to the side of heat. After adjustment five uniform and successive deflections were obtained with the instrument, as the light of the star was allowed to fall upon the vulcanite or was screened off from it. During the totality the tasimeter was placed in a double tin case containing water at the temperature of the air. At the instant when the sunlight disappeared the spot of light was passing slowly towards the side of cold. When the screen was withdrawn so as to permit the edge of the corona to fall upon the vulcanite, the spot of light came to rest and then moved gradually toward the side of heat, and in four or five seconds passed entirely off the scale.

The general conclusions reached by Dr. Draper from the results obtained were that the corona of the sun shines by light reflected from the solar mass by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1,474 matter and hydrogen.

In 1874, upon the organization of the United States Commission to observe the transit of Venus, Professor Draper's achievements in celestial photography pointed him out at once as the person best suited to organize the photographic section of the expedition, and he was accordingly appointed in February the Director of the Photographic Department. In the early part of April he went to Washington, entered into the work most heartily at once, and during three entire months devoted himself almost without intermission to the work of devising improved methods, of testing instruments and materials, and of instructing those who were to use these methods how to obtain the best results with them; declining subsequently to accept compensation for the time thus spent. Although his duties at home prevented him from joining any of the expeditions thus sent out, yet so conspicuously valuable had been his services and so largely instrumental had they been in making the transit observations a success that, upon the recommendation of the Commission, Congress ordered a special gold medal to be struck in his honor at the mint in Philadelphia. This medal is 46 millimeters in diameter. Upon the obverse it has the representation of a siderostat in relief, with the motto, "*Famam extendere factis hoc virtutis*

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opus." Upon the reverse is inscribed the words: "Veneris in sole spectandæ curatores R. P. F. S. Henrico Draper, M. D., Dec. VIII, MDCCCLXXIV," together with the motto, "Decori decus addit avito."

Toward the close of 1868 Professor Draper became greatly interested in a project to establish an observatory in Central Park, and had numerous consultations upon the subject with Mr. Andrew Green, at that time the controller, and Mr. Vaux, the architect of the park. In December the park was visited and the site of the observatory selected. It was a knoll at about 105th street, 130 feet above tide water, and commanding an unobstructed view. Dr. Draper was appointed the director of this new observatory, and plans were arranged for securing the \$250,000 which it was estimated would be required for carrying the project into operation.

Besides that which was spent in scientific work, Dr. Draper's time was largely occupied with his duties as an instructor. We have already seen that as early as 1858 he had been appointed on the medical staff of Bellevue Hospital and had served in that position for eighteen months. As Professor of Natural Science in the Academic Department of the University of the City of New York from 1860 until 1882 and as Professor of Physiology in the Medical Department of the University from 1866 until 1873, he was called upon to deliver numerous lectures, and as Dean of the Medical Faculty much time was necessarily given to the management of the business affairs of the college. It is to his signal ability in this position, aided by a liberal use of his own private means, that the college is largely indebted for the promptness with which a new building was provided and its lectures were resumed after the destruction by fire of its old building on Fourteenth street. After the death of his father the chief inducement for him to retain his connection with the University no longer existed; so that, although elected to the chair thus vacated, he discharged its duties only until the close of the current academic year.

Still a third sphere of labor made its demands upon Dr. Draper's time. In 1867 he had married Mary Anna, the accomplished daughter of Courtlandt Palmer, of New York. Upon Mr. Palmer's death, in 1874, Dr. Draper became the managing trustee of a large estate; and, with his characteristic energy and ability, entered at once upon the task of reducing it to a basis of maximum production with the minimum amount of attention. The responsibility which thus rested

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upon him, the harassing demands of tenants, the endless details of leases, contracts, and deeds, and the no less annoying complications of necessary lawsuits worried him incessantly. Had it not been for his unsurpassed business capacity he might have been overborne by this excessive strain. But he was equal to the demands made upon him, and within a few years order had come out of confusion, and a few hours spent daily at his office enabled him to maintain the affairs of the estate in a satisfactory condition.

Early in February, 1862, Dr. Draper was commissioned surgeon of the Twelfth Regiment New York State Militia, and on the 6th of June he left with the regiment for three months' service in Virginia. He was mustered out of the United States service on the 7th of October, and returned to New York sick with the Chickahominy fever, contracted in the swamps bordering the Monocacy river. In 1876 he served as one of the judges in the photographic section of the Centennial Exhibition. Already, in 1875, Dr. Draper had been elected a member of the *Astronomische Gesellschaft*. In 1877 he was made a member of the American Philosophical Society in Philadelphia, and the same year was elected a member of the National Academy of Sciences. In 1879 he was elected a Fellow of the American Association for the Advancement of Science. In 1881 the American Academy of Arts and Sciences in Boston enrolled his name among the list of its associate fellows. In 1882 he received the degree of doctor of laws almost simultaneously from the University of Wisconsin and from his *alma mater*, the University of the City of New York.

For several years it had been Dr. Draper's custom, in order to get rest from the severe labors of the year and to fortify his constitution for the winter strain, to join his friends, Generals Marcy and Whipple, of the United States Army, for a few weeks' hunting in the early fall in the Rocky Mountains. These expeditions he enjoyed greatly. He was an enthusiastic sportsman and a capital shot, and he entered upon the chase with as much relish as he took a stellar photograph. It was while out upon such an expedition in 1877 that he made the important observations on the suitableness of the air of that region for astronomical investigations. The party in 1882 left New York on the 31st of August, went by rail to Rock Creek, on the Union Pacific Railway, and from there went north in the saddle, reaching Fort Custer, on the Northern Pacific Railway, near the middle of October. During the two months of their ab-

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sence the party rode fifteen hundred miles on horseback, as Dr. Draper estimated. When above timber line, early in October, they encountered a blinding snow-storm with intense cold, and were obliged to camp without shelter. On the 25th of October Dr. Draper reached New York. Ordinarily he returned refreshed and invigorated with the splendid exercise of the trip; but this year the distance traveled seemed to have been too great, and this, together with the hardships encountered, seemed to have wearied him. Pressure of delayed business awaited him and occupied his time at once. Moreover, the National Academy of Sciences was to hold its November meeting in New York, and Dr. Draper was to entertain them as he had always done. This year the entertainment was to take the form of a dinner. In order to offer a scientific novelty, he determined to light the table with the Edison incandescent light, the current being supplied from the dynamo machine in his laboratory; but the source of the power being a gas engine, and therefore intermittent in its action, a disagreeable pulsation was observable in the light. To obviate this he contrived an ingenious attachment to the engine whereby, at the instant at which the speed was increased by the explosion of the gas in the cylinder, a lateral or shunt circuit, the resistance of which could be varied at pleasure, should be automatically thrown in. With his admirable mechanical skill, Dr. Draper extemporized the device from materials at hand and found it to work perfectly. The dinner was given on the evening of November 15th, and was one of the most brilliant ever given in New York; about forty academicians, together with a few personal friends, sitting at the table. Dr. Draper's overwork, however, now told upon him; slightly indisposed as he had been before, he was unable to partake of food and a premonitory chill seized him while at the table. As soon as the dinner was over and the guests had departed he took a hot bath, thinking thus to throw off the attack. But while in the bath a second and more severe chill of a decidedly congestive type attacked him, and it was only with the greatest difficulty that he could be carried to his bed. His warm friend and former colleague, Dr. Metcalfe, who had been a guest at the dinner, was at once summoned and pronounced the attack double pleuritis. The best of treatment and the most careful nursing seemed for two or three days to be producing an effect for the better. But on the Sunday following pericarditis developed, and

he died about four o'clock on the morning of Monday, the 20th of November, 1882.

Viewed from whatever standpoint, the life of Henry Draper appears as successful as it was earnest, honest, and pure. His devotion to science was supreme; to him no labor was too severe, no sacrifice too great, if by it he could approach nearer the exact truth. The researches he had already made and, much more, those he had projected involved the largest expenditure of his time and means. But such was his delight in his scientific work and such his enthusiasm in carrying it on that he was never happier than when hardest at work in his laboratory; never more cheerful than when most zealously laboring with his superb appliances. Dr. Draper's abilities, too, were many-sided. He was eminent in astronomy, in physics, in chemistry, and in physiology. He possessed exceptional skill as a mechanic, as the wonderfully accurate mountings of his telescopes abundantly prove. Moreover, he was quite as distinguished as a teacher as he was as a scientific investigator. His lectures were simple, clear, and forcible. They held the interest of the classes he instructed and awakened their enthusiasm, while they enriched the student's store of knowledge and strengthened his powers of observation and of reasoning. In the laboratory he was keen, thorough, and impartial, while at the same time considerate and helpful, ever striving to encourage honest endeavor and to assist the earnest worker. As a business man he is said to have had no superior in the city of New York. In social life he was brilliant, entertaining, companionable. He made life-long friends often at the first contact by the charm of his presence and the suavity of his manner.

Professor Draper had not published very extensively at the time of his death. This is the more remarkable as he was fond of writing, a trait no doubt inherited from his father. A list of his publications is appended to the present memoir. There can be little room to doubt that had he not been cut down so abruptly in the midst of a host of projected investigations, the world would have been enriched during the succeeding twenty years with a wealth of discovery almost unparalleled.

To indicate the esteem in which Dr. Draper was held by his confrères in science the following passage may be quoted from an excellent biographical notice of him written by Professor Young, of Princeton: "In person he was of medium height, compactly built,

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with a pleasing address and a keen black eye which missed nothing within its range. He was affectionate, noble, just, and generous; a thorough gentleman, with a quick and burning contempt for all shams and meanness; a friend most kind, sympathetic, helpful, and brotherly; genial, wise, and witty in conversation; clear-headed, prudent, and active in business; a man of the highest and most refined intellectual tastes and qualities; a lover of art and music, and also of manly sports, especially the hunt; of such manual skill that no mechanic in the city could do finer work than he; in the pursuit of science able, indefatigable, indomitable, sparing neither time, labor, nor expense."

"Excepting his early death, Dr. Draper was a man fortunate in all things: in his vigorous physique, his delicate senses, and skillful hand; in his birth and education; in his friendships, and especially in his marriage, which brought to him not only wealth and all the happiness which naturally comes with a lovely, true-hearted, and faithful wife, but also a most unusual companionship and intellectual sympathy in all his favorite pursuits. He was fortunate in the great resources which lay at his disposal and in the wisdom to manage and use them well; in the subjects he chose for his researches, and in the complete success he invariably attained."

In closing this biographical notice of Henry Draper, I beg my colleagues of the Academy to remember that his life and work have been viewed by me through the medium of a warm personal friendship. To me Professor Draper was indeed a man among men, a scientist of the highest type. Stricken down as he was, in the midst of his life-work, at the early age of forty-five, the bright promise of his noble life was left unfulfilled. What brilliant researches in his favorite science he might have made we can never know. But with a mind so richly endowed and so thoroughly trained, with an experimental ability as earnest as it was persistent, with facilities for investigation which were as perfect as they are rare, with abundance of time and means at his disposal, and, above all, with a devoted wife, who keenly appreciated the value of his scientific work, was ever at his side as his trusty assistant, and always shared in the glory and honor of his discoveries, we may be sure that had he been permitted to reach the age of his honored father results would have been reaped by his labors which would have added still brighter lustre to the science of America.

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LIST OF HENRY DRAPER'S PUBLISHED PAPERS.

1. On the Changes of Blood Cells in the Spleen. *New York Journal of Medicine*, III, v, 182-189, Sept., 1858.
2. On a New Method of Darkening Collodion Negatives. *Am. J. Photog.*, II, i, 374-376, May, 1856.
3. On a Reflecting Telescope for Celestial Photography. *Rep. Brit. Assoc.*, 1860, II, 63-64.
4. Article "Photography." *New American Cyclopaedia*. New York, 1863.
5. On an Improved Photographic Process. *Am. J. Photog.*, II, v, 47, Feb., 1862. (*London Phot. News*.)
6. On a Silvered Glass Telescope and on Celestial Photography in America. *Quar. J. Sci.*, i, 381-387, April, 1864.
7. On the Photographic Use of a Silvered Glass Telescope. *Phil. Mag.*, IV, xxviii, 249-255, 1864.
8. On the Construction of a Silvered Glass Telescope 15½ inches in Aperture, and its Use in Celestial Photography. *Smithsonian Contributions*, XIV, Part II. July, 1864.
9. Petroleum; its Importance, its History, boring, refining. *Quar. J. Sci.*, ii, 49-59, 1865. *Dingler's Polyt. J.*, clxxviii, 107-117.
10. American Contributions to Spectrum Analysis. *Quar. J. Sci.*, ii, 395-401, 1865.
11. A Text-book on Chemistry. New York, 1866.
12. Report on the Chemical and Physical Facts collected from the Deep Sea Researches made during the voyage of the School-ship Mercury. *Rep. Comm. Pub. Charities*. New York, 1871.
13. On Diffraction Spectrum Photography. *Am. J. Sci.*, III, vi, 401-409, Dec., 1873. *Phil. Mag.*, IV, xlvi, 417-425. *Ann. Phys. Chem.*, cli, 337-350, 1874.
14. Astronomical Observations on the Atmosphere of the Rocky Mountains, made at Elevations of from 4,500 to 11,000 feet, in Utah and Wyoming Territories and Colorado. *Am. J. Sci.*, III, xiii, 89-95, Feb., 1877.
15. Photographs of the Spectra of Venus and α Lyrae. *Am. J. Sci.*, III, xiii, 95, Feb., 1877. *Phil. Mag.*, V, iii, 238.

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16. Discovery of Oxygen in the Sun by Photography, and a new Theory of the Solar Spectrum. *Proc. Am. Phil. Soc.*, July, 1877, 74-80. *Am. J. Sci.*, III, xiv, 99-96, 1877.

17. Article "Speculum." *New Universal Cyclopedia*. New York, 1878.

18. Observations on the Total Eclipse of the Sun of July 29th, 1878. *Am. J. Sci.*, III, xvi, 227-230, Sept., 1878. *Phil. Mag.*, V, vi, 318-320.

19. On the Coincidence of the Bright Lines of the Oxygen Spectrum with the Bright Lines of the Solar Spectrum. *Am. J. Sci.*, III, xviii, 262-277, Oct., 1879. *Monthly Not. Astr. Soc.*, xxxix, No. 8.

20. On Photographing the Spectra of the Stars and Planets. *Am. J. Sci.*, III, xviii, 419-425, Dec., 1879. *Nature*, Nov. 27, 1879.

21. On a Photograph of Jupiter's Spectrum showing Evidence of Intrinsic Light from that Planet. *Am. J. Sci.*, III, xx, 118-121, Aug., 1880. *Monthly Not. Astr. Soc.*, xl, 433-436, May, 1880.

22. On Photographs of the Nebula in Orion. *Am. J. Sci.*, III, xx, 433, 1880. *Phil. Mag.*, V, x, 388. *Comptes Rendus*, xci, 688; xcii, 173, 964.

23. On Photographs of the Spectrum of the Comet of June, 1881. *Am. J. Sci.*, III, xxii, 134, 1881.

24. On Photographs of the Spectrum of the Nebula in Orion. *Am. J. Sci.*, III, xxiii, 339-341, May, 1882.

MEMOIR
OF
SPENCER FULLERTON BAIRD.
1823—1887.

BY
JOHN S. BILLINGS.

READ BEFORE THE NATIONAL ACADEMY, APRIL 17, 1889.

BIOGRAPHICAL MEMOIR OF SPENCER F. BAIRD.

A memorial of a scientific man, prepared for a scientific association, does not, as a rule, refer so much to the personality of the individual, or to his biography in the ordinary sense of the term, as to his contributions to the common stock of existing knowledge, and to the relations he has had to, or the influence he has exerted upon, the progress of science. It does not often consider him from the social or family point of view, and in fact, in the great majority of cases, there is little to be said about the quiet, uneventful life of the scientific worker. The particular thread of warp or woof formed by his existence in the variegated tissue of human progress is often not conspicuous during his lifetime, though it is usually of fast colors; and when one comes to trace it carefully it is often surprising to see what an important part of the pattern it makes, and how it expands and ramifies as the web goes on.

Occasionally, however, such a memorial has to treat of a man distinguished not only for his own scientific work and original contributions to knowledge, but for his strong and wide-spread influence upon the work of others, through his connection with the organization and development of institutions which every one recognizes to be of national interest and importance. In such a case we want to know, not merely the little string of dates which mark the beginning, turnings, and end of such a life, or the list of the books and papers which he wrote, but how he managed men, avoided controversy, obtained means, and overcame obstacles—in short, the methods as well as the results. It is of such a man that I have been requested to prepare a memorial for the National Academy, and one of the difficulties which I have met with in attempting it has been excess rather than deficiency of material.

The biographical sketches and eulogies of Professor Baird, which have already appeared, prepared by intimate and admiring friends, give full details of his published contributions to science, and set forth the breadth and depth of his knowledge, the amount of his labor, and the perfection of his character, with such feeling and eloquence that any subsequent account must either simply copy

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them or seem cold and critical by contrast. It has, therefore, seemed to me that I could best use the limited time at my disposal by omitting biographical and bibliographical details to a great extent, and considering mainly some of the indirect results which he produced, and some of the causes of the powerful influence which he wielded.

SPENCER FULLERTON BAIRD was born February 3, 1823, at Reading, Pennsylvania. His remote ancestors were English, Scotch, and German—an excellent combination for both business and science. His father was a lawyer, and, as is not unusual in this profession, was also a lover of nature, and his tastes strongly influenced those of his son. Spencer entered Dickinson College, at Carlisle, Pennsylvania, in 1836, being then thirteen years old, and graduated in arts in 1840, at the age of seventeen. He then began the study of medicine and attended one course of lectures, but was drawn aside from this profession by his interest in natural history. He had formed the acquaintance of Audubon while yet a student, and in 1842 received from him a part of his collection of birds, including many types of new species. The next three or four years were spent mainly in long tramps through the country collecting specimens, and in the study of systematic zoology.

In 1845 he was chosen Professor of Natural History in Dickinson College, and the next year became also Professor of Chemistry. He was strongly influenced by Agassiz, who came from Switzerland in 1847, and who, very soon after his arrival, projected a work to be carried on with Professor Baird on the fresh-water fishes of the United States, which, however, was never published.

In 1848 he obtained from the Smithsonian Institution one of its first grants for the promotion of original research, which was to be applied to the exploration of bone caves in Pennsylvania. These caves, and especially some in the vicinity of Carlisle, always had a great attraction for Professor Baird, and he often spoke of his desire to go back and take a rest from routine work by continuing their exploration.

In 1850, mainly through the influence of the late George P. Marsh, he was appointed Assistant Secretary in the Department of Natural History of the Smithsonian Institution, which had been organized in 1847. Professor Baird brought with him his extensive collections, chiefly of skins of mammals and birds, of birds' nests and eggs, and of alcoholic specimens of reptiles and fishes, with

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some skeletons and fossils. At that time the Institution had a few boxes of minerals and plants, which were of little importance. These collections of Professor Baird formed the nucleus of the present National Museum, which was at last definitely established in 1857 by the formal transfer of the collection of the Wilks Expedition from the Patent Office, and which has been organized and managed on the methods devised by the young naturalist for his own private collection.

When Professor Baird was appointed Assistant Secretary the immediate duty to which he was assigned was the charge of the library and the department of exchanges. His tastes and training were very different from those of Professor Henry. He cared little for physical or experimental science, was no mathematician, and had no taste for laboratory work. He was a naturalist, an explorer, and a collector, taking immense pleasure in out-of-door work and in getting his materials at first hand, and almost equal pleasure in classifying and preserving them. It was the possibility of creating a great museum of natural history that induced him to come to the Smithsonian, and he never lost sight of this object; but for a long time he had to work largely by indirect methods. He did not directly oppose the policy of Professor Henry, and always worked harmoniously with him, but he lost no opportunity of increasing the collections, and constantly urged that the best way to induce Congress to grant the means of caring for such things was to accumulate material worth caring for until its amount and value should be such that public opinion would demand ample accommodation for it.

So early as 1853 we find him writing to his friend, Mr. Marsh, about a scheme for a national museum, and a year later he got so far as to consider plans and size of buildings, having in view apparently something like the Crystal Palace. He was not working aimlessly all those years. He could not have what he wanted just then, but he had faith in the future, and meantime went on with his duties, which Mr. Marsh characterized as "answering of foolish letters, directing of packages to literary societies, reading of proof-sheets, and other mechanical operations pertaining unto the diffusion of knowledge."¹

In 1871 he was appointed to the position of United States Commissioner of Fish and Fisheries, an office specially created for him,

¹ *Life and Letters of George P. Marsh.* N. Y., 1888. Vol. i, p. 262.

since the act required that the commissioner should be taken from the civil officers or employés of the Government; should be of proved scientific and practical acquaintance with the fishes of the coast, and should serve without additional salary. This position opened to him a field of work which, in most respects, precisely suited his tastes; it gave him opportunities of making immense collections, of which he was not slow to avail himself, and there was enough utilitarianism in the work, that could be appreciated by every one, to make it easy to secure ample appropriations from Congress for carrying it on. He gradually became more and more absorbed in this part of his occupation, and during the last four or five years of his life thought of but little else.

In his earlier years of study Professor Baird did a vast amount of bibliographical research, especially with reference to synonymy and the geographical distribution of species and genera, and the results of this are evident in his published works, although these do not by any means include all of his memoranda and notes upon such points, which were wonderfully complete, considering the difficulties which he must have had in getting access to the voluminous literature of the subject. No doubt the library of the Academy of Natural Sciences of Philadelphia was the chief source from which he drew for this purpose.

The most important field of labor of Professor Baird during the first twenty years of his service in the Smithsonian Institution was in connection with the various Government explorations and surveys which were from time to time sent out. In most cases he drew up the descriptions for the natural history of the results of such expeditions, practically selected the collectors, and edited a large part of the zoological reports. In the Annual Report of the Smithsonian he described the work of such expeditions during the year, and this is the only continuous record which we possess of what the Government has done in this direction. It was mainly through these channels that he secured a large part of the material for the National Museum, and, at the same time, induced Congress to make increasing appropriations for its care, until at last the large gifts of specimens which came from the Philadelphia Exposition of 1876 led to the fulfillment of the plans at which he had been working for a quarter of a century, in the granting by Congress of funds for the construction of a separate building, and to liberal appropriations for obtaining, preserving, and displaying specimens.

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Upon the death of Professor Henry, in 1878, Professor Baird succeeded him as Secretary of the Smithsonian Institution. He effected some changes in its policy, more especially in the distribution of its funds, which will be referred to hereafter, but in the main his thoughts and labor continued to be devoted to the Fish Commission and the National Museum.

He was a man of fine physique, which had been well developed by his early habits of pedestrianism, but great muscular development has its inconveniences and dangers if its demands for exercise are not complied with. Muscle and nerve form one organ, the proportions of which, if normal, are adapted to each other, to the appetites which belong to them, and to the organs of circulation. If the muscle does not receive its due amount of exercise it not only degenerates, but throws extra work on other organs. When Professor Baird, as a young man, walked from forty to fifty miles a day for two weeks in succession he not only developed the muscles immediately concerned in locomotion, but also a heart of increased size and power, capable of dealing with the rapid circulation of blood demanded by it, and an appetite and digestion suited to provide the force exerted.

The heart built for its work not only muscle, but an enlarged framework of fibroid tissue about the mouth of the great vessels suited to withstand the constant pull and strain. When he changed his mode of life to almost constant desk work the muscle diminished, but the scaffolding did not also lessen in the same proportion, for this fibroid connective tissue is more permanent. So also the appetite for food continued, but the supply was no longer consumed by muscular effort, and had to be gotten rid of through other channels, which had too much work to do. Before he had been five years in the Smithsonian his heart began to trouble him, especially after clerical work,¹ and this condition slowly progressed, aggravated by his mode of life, until it gave him a weak, flaccid heart, stretched on a framework too big for it and unable to respond to any sudden demand for extra work. When at last, twenty years later, he perceived that the "edge of the dark" had passed over him it was practically too late to change his mode of life, his interests, or his work; he could only make provision for

¹ See Report No. 141, 33d Congress, 2d session, House of Representatives, page 80.

assistants and for a successor, avoid effort and strain, and seek relaxation and rest in that branch of work which most interested him—namely, the Fish Commission. When the shadows grew so deep that he knew the inevitable end was near, he went to the laboratory he had created at Woods Holl, and there, surrounded by fellow-workers and friends, he died August 19, 1887, at the age of sixty-four.

The characteristics of Professor Baird which I propose to notice are his unusual qualifications as an original investigator in natural history, his tact and skill in subordinate positions, his capacity for managing men and for organization and administration when placed in charge, his persistent labor, and his modesty and usefulness in his devotion to the particular branch of science in which he was interested.

As an original investigator he did an enormous amount of work during the twenty-six years from 1843 to 1869, which he specially devoted to study of the mammals, birds, reptiles, and fishes of North America. As to quantity, Professor Goode has filled a volume of nearly 400 pages with the bibliography of his published writings up to 1882, including 1,063 titles, of which about 200 relate to formal contributions to scientific literature, while in addition to this he furnished much material in the shape of notes of unfinished work and suggestions for farther inquiry to be utilized by others. His reputation as a scientific naturalist was established by the publication of Vols. VIII and IX of the Pacific Railroad Reports, containing the descriptions and classifications of the mammals and birds of North America. These contain not merely descriptions of a large number of new species, but a general revision of the classification and nomenclature, and the principles upon which these were founded have for the most part stood the test of time, showing the keenness of his insight into what may be called fundamental morphology. These large volumes are still the standard works on the subjects of which they treat, and the additions which have been made to them are mainly the work of his own pupils and in his own manner and methods.

The circumstances of his position confined his work mainly to descriptive morphology, systemization, and nomenclature, but his early training as a field naturalist and collector and his keen powers of observation of the actions of living animals in the wild state entirely removed him from the category of the mere species

describers. For example, when, as one of the Trustees of the Corcoran Art Gallery, he was called on to see Richard's picture of a scene on the New Jersey coast obtained for that collection, it was not the rush of the waves, the seething spray, the dull glitter of the lonely beach, or the flying drift of the clouds, which are the chief features of the painting, which caught his attention, but the fact that the gulls were depicted with a plumage which these birds do not have at the season represented, and this error to a considerable extent spoiled the picture for him. The determinations of genera and species made by him are founded mainly on bones and skins, which formed the bulk of the material available at the time, and it was not until the laboratory of the Fish Commission was organized that he was able to do much in embryology; nevertheless, he personally made many dissections of smaller animals and reptiles, such as are preserved in alcohol, and left many unpublished notes of the results. It is not an easy matter to estimate fairly the importance of this kind of work and the influence which it has on scientific progress and general culture, and it is very likely to be either under or over valued by those who are not familiar with the study of living organisms. Classification, description, and naming of the different forms are the essential foundations of scientific biology, for until this has been done identification of particular forms is either difficult or impossible, co-operative work on the part of scattered students is greatly restricted, and broad generalizations can only be put in the form of theories and conjectures. Such work as was done by Professor Baird in this direction gives a starting point to many observers and investigators in different localities, stimulates farther inquiry, and, when done on the extensive scale on which he did it, based on the examination and comparison of a large number of specimens from widely different localities, exercises a powerful influence for years to come on lines of exploration, collection, and critical research. To those who have never tried it, it may seem an easy matter to sort out specimens of different kinds when a large number are brought together, or to prepare descriptions sufficient to enable another man to identify his specimen; but in reality it requires not only much experience and careful study, but a certain aptitude, power of grasping salient points, and of putting aside unessentials such as is rarely possessed by any man.

On the other hand it should be remembered that this is foundation work only, and that its importance and value depends on the

structure to be reared upon it, upon the higher generalizations with regard to the relations of size, color, form, etc., to each other, to the environment, and to the functions of the living organism, and that mere names and descriptions, however well done, are only the bricks with which true science builds.

It is, however, usually true that the man who can rightly classify and describe can also best deduce conclusions from the results of such studies. As an example of Professor Baird's ability in this direction, take his paper on the distribution and migrations of North American birds, published in *Silliman's Journal* for 1866. In this paper he maps out the country into regions corresponding to the distribution of different kinds of birds, discusses the relations of these regions to surface topography, altitude, temperature, mountain chains, etc.; points out that there are certain correspondences in the distribution of reptiles and fishes, and draw the conclusions that North American birds of wide distribution in latitude, whether migrants or residents, will be found to be larger the higher the latitude of their place of birth, and that this is also true within certain limits of mammals and even of man; that specimens from the Pacific coast are apt to be darker in color than those from the interior, and that specimens from near the line of junction of two well-marked provinces or regions often show the influence of hybridization. When he comes to discuss migrations it is in their relations to the laws of the winds of the Northern Hemisphere that he studies them, and concludes that the transfer of American birds to Europe is mainly due to air currents. Finally he says, and this is the germ of the Fish Commission, which came five years later, "a region deprived of its spring birds or fishes by extermination will only be filled up again in the course of a long period of time. The result, however, can be greatly accelerated by artificial propagation in the places to be supplied."

Professor Baird did not himself produce much of this sort of scientific literature, for he had not the opportunity, since at the very period of his career when he was best fitted to make such studies he had to give almost his whole time and energy to routine administrative duties; but this paper alone is sufficient evidence of his capacity for generalizing from a series of isolated facts.

With regard to his deportment in the subordinate position of Assistant Secretary of the Smithsonian, especially in the earlier years of his connection with that Institution, it is noteworthy that,

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while he did not yield his opinions or change in the least in his tastes or preferences for natural history investigations, he did thoroughly well the work which was assigned to him, and never allowed his personal pleasure to stand in the way of carrying out the plans of the Secretary and of the Regents as far as they related to the division under his charge. For a time he threw all his energy into the organization and expansion of the Bureau of Exchanges, so much so that his friend, George P. Marsh, in some of his charming letters to him, half jokingly and half seriously remonstrates with him on his enthusiasm over the increasing number of boxes and packages sent out and received, the number of letters he was writing, etc., and expresses doubts and fears lest he should become a first-rate packing and shipping clerk. Professor Baird himself, however, never shirked this mechanical drudgery, never publicly bewailed that he could not do just what he wanted to do, never smote his breast and rent his garments and called on the world to witness that he was a remarkable scientist, and that as such he ought to be made much of and be allowed to blow his own horn exclusively, even if he did receive pay for doing something else.

The system of Smithsonian exchanges was practically an extension of those carried on by the American Philosophical Society and the American Academy of Arts and Sciences for some forty years, and its establishment was one of the first definite acts of the Smithsonian Institution. Previous to this there had been some exchanges made under an act of Congress approved July 20, 1840, and also by the National Institute. When the Smithsonian was organized, one of the most urgent demands made upon it was that it should take charge of, expand and improve this system of exchanges. It was difficult for the newly formed and rapidly organizing scientific and educational institutions in this country, and still more so for isolated and comparatively unknown workers in special fields, to establish communication and exchange with the scientific institutions and students of other countries; there was no organized express system, the custom-house delays were long and vexatious, and there was little certainty that packages which could not be sent by mail would ever reach their destination.

Mainly through the energy and incessant labor of Professor Baird, the Smithsonian was soon able to render most important aid in this direction; it not only had its own agents in all the principal cities of Europe, but American ministers and

consuls everywhere were induced to interest themselves in receiving and forwarding packages; the Treasury Department allowed its parcels to be forwarded without delay; many lines of shipping and railroads gave it free transportation; and so it soon came to pass that scientific men in this country had better facilities for exchange with Europe than European countries had with each other. All this was done with comparatively small expenditure, the average annual cost to the Institution prior to 1867 being only about \$2,000. In 1867 Congress placed the system of exchange of Government publications under the charge of the Smithsonian, and the expense rapidly increased, so that for the years 1878-'80 they averaged \$10,000, or nearly one-third of the entire revenue of the Institution. In 1881, through the influence of Professor Baird, Congress granted \$1,500 towards the cost of exchanges, and this sum was rapidly increased in succeeding years to \$15,000, as was fair and just.

But, while doing this routine work, and doing it so well as to lift it out of the region of routine, Professor Baird never for a day lost sight of his plan for a National Museum, never neglected a possible opportunity to acquire material for it, never ceased the studies which were his chief delight, and yet never came into unpleasant collision with his official superiors who had other interests and views. This was not because he concealed his opinions, but because he did all that was given to him to do and more, and used his remarkable power of managing men in behalf of his official work as well as of that which he had more immediately at heart.

Of his capacity for organization and administration, the National Museum and the work of the Fish Commission are sufficient evidence. He had the full confidence of those very distrustful bodies of men, the Committees on Appropriations of the House and Senate, and while he never seemed to be urgent in his demands, he almost invariably obtained from Congress all that he desired. One reason for this was that he did not try to go too fast, and managed to educate public opinion so that the demands when made met with almost unanimous support. In fact he offered suggestions rather than demands, and preferred to have others take up the suggestions and deal with them as if they were their own original ideas, while he remained quietly in the background ready to furnish information when desired, but taking no apparent part in discussion or controversy, and absolutely indifferent as to who should

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have the credit, so long as the work was done. The wide-spread net-work of Smithsonian correspondents which he had established was of great use in this preliminary education of public opinion. The fact that he always got the fullest money's worth for his expenditure was also well known to Congress. A distinguished Senator remarked, "I am willing to vote the money asked for by Professor Baird, for he will get two dollars' worth for every dollar we give him—one-half by direct purchase and one-half by gift." He was in fact a hard bargainer in exchanges, aiming to get as much and give as little as possible; but this was for the Government collections as a whole and not merely for the National Museum, and least of all for himself. For example, in dealing with the Army Medical Museum there was no chaffering; he gave more than he got, and willingly, because he considered the Medical Museum as a valuable branch of the National Collection; the only point he insisted on was that whatever came to the Medical Museum in the shape of dress, weapons, or utensils of savage life, be it much or little, should be sent to the National Museum, where it could be better used and placed.

His personal presence, including his size, his uniformly quiet, simple manner, evident honesty and sincerity of purpose, and perfect readiness to answer all questions had a powerful effect on the committees. Another cause of his influence was the fact that a very large proportion of the legislators with whom he had to deal were men whose youth had been spent in the country, and who as boys had become somewhat interested in the animals, birds, and fish which they had hunted or entrapped. In Professor Baird they found a man who recalled some of the pleasantest days of their youth, who could answer many of the questions which had puzzled them in days gone by, who was a hunter and a fisherman as well as a collector, and who could make his plans and wishes not only intelligible but full of interest to them. He had good soil in which to plant, and he was a skillful cultivator; no wonder that the crop was large. Take, for instance, the growth and development of the Fish Commission, which originated in a holiday season spent in the vicinity of Woods Holl, where he amused himself with the rich marine fauna of the vicinity and with talks with the fishermen round about. When he gathered and repeated the stories of the growing scarcity of some important food fishes he had plenty of interested hearers in the Senate and in the House, men who were

fishermen themselves and were quick to grasp the idea of the possible benefits to be derived from a careful study of the causes of this scarcity. Argument and solicitation were almost unnecessary, the mere suggestion was sufficient, and after the publication of his first report the increase of the work was rather forced upon, than demanded by him.

With regard to those not immediately connected with the institutions of which he had charge, he had grasped the open secret that one makes friends quite as much by asking and receiving as by giving, provided it be clear that the asking is not for personal benefit. Most men, in this country or abroad, were glad to reply to Professor Baird's innumerable letters of inquiry, to obtain for him this or that particular bird or mammal or reptile. He made them feel personally interested in the completeness of the collection, that it was partly theirs, and that they should have full credit for their work. By his own immediate staff of assistants he was respected, trusted, and beloved to an unusual degree. He exacted full work and was rigidly economical in salaries as in all other expenditures, and personal feelings were never allowed to interfere with what he believed to be for the best interests of the work under his direction; but changes in the staff were very few, every one was sure of the most cordial assistance in his work if he showed zeal and ability, and his charity towards short-comings and moral delinquencies was, if anything, excessive. He let his assistants work in their own way, ready to help or advise when called on, but never fussily interfering or dictating minutiae of methods; so that each one felt that he was to a great extent independent, and therefore, to a corresponding extent, responsible for good work and plenty of it, to which the constant example set by himself was a powerful stimulus.

He was not an active worker in any of the scientific societies with which he was connected. He did not like to read papers, or to take part in formal discussion, and, above all things, he hated the necessity of sitting still for two hours with nothing to do but listen to papers which in the great majority of cases had no relation to his work, under which circumstances he usually went to sleep. Nevertheless, he was always interested in the prosperity of such societies, always ready to furnish materials and suggestions to others who would make use of them in papers or discussions, and in fact to do almost anything except to attend, to talk, or to fill any official position in them. It is said that the only time he was

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ever seen on the platform in a public assembly was when he received the honorary degree of LL. D. from Harvard. The only communication which he presented to the National Academy was the paper on the distribution and migration of North American birds, read in 1865, to which reference has been made.

I have already alluded to the views of Professor Baird with regard to the use to be made of the Smithsonian funds, and these are clearly indicated in the change which took place in the character of the expenditures after he became Secretary. The average annual expenditure for original research for 1850 to 1877 was a little over \$2,000 a year. After Professor Baird took charge, in 1878, it fell to \$802.80 for each of the next three years, and then ceased entirely. In like manner the expenditures for apparatus and for laboratory were soon cut off after 1878, while, on the other hand, the expenditures for collection and for explorations more than doubled, being in the main for the benefit of the National Museum. The character of the publications also changed; the cost of the Smithsonian contributions to knowledge, which from 1870 to 1877 averaged \$8,140.71 per annum, fell to an average of \$3,270.83 annually during the next ten years, while the cost of the annual reports more than doubled in the same time. The physicist, the chemist, and the naturalist will differ in opinion as to the wisdom of this change, but all must admit that it was no ordinary amount of tact, of management, and of executive ability which so rapidly effected such a change in the policy of the Regents of the Institution.

The relations of the Smithsonian Institution and of the National Museum to each other, and to science and scientific man in this country, have been and still are very intimate; the work of their founders and organizers forms an integral part of the history of science in America; they have had a powerful influence in educating public opinion and in shaping the legislation which has provided for skilled explorations and investigations in so many branches of the Government, and everything connected with them and their directors must always be of special interest to this Academy.

In the light of subsequent events it may be said that the course pursued by the Smithsonian authorities has been in the main a wise one. It would not have been well if the whole income of the trust had been turned at once into one channel, and it is best turned in different directions at different times. No better work can be done

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by such an untrammelled trust than to commence investigations, collections, or publications, do enough to demonstrate their practicability and utility, educate popular opinion to their support, and then leave them to the care of others to develop by gemmation and not by segmentation. There is now no need for the Smithsonian to expend any of its limited funds on books or on specimens of art or natural history, but it should not be forgotten that this is so because for a number of years it did make such expenditures, and thus stimulated and made possible the national libraries and museums of to-day, and in this part of its work Professor Baird stands pre-eminent.

It is also well that we should have, not a National Library and a National Museum, but National Libraries and Museums. The scheme of the ambitious collector to gather every book into one library, or a specimen of everything relating to science or art into one museum, is not a wise one. Differentiation is as necessary as accretion for development and production of the greatest utility; true wisdom consists in so adjusting these processes as to secure the greatest interest in and utility of the several groups.

The provision by Congress that there should be a chemical laboratory is a sign of the times; they did not know anything about any other kind of laboratory, not even a physical one. Under the same circumstances to-day a biological or a physical laboratory would be prescribed, if any. When the Smithsonian began its work some wanted it to form a great library, some to create an art gallery, some to make a museum, others to collect meteorological data. As matters now stand, we have an art gallery whose income is greater than that of the Smithsonian, a meteorological bureau which costs about \$900,000 a year, the Congressional Library with an annual appropriation of \$71,000 (which will no doubt be much increased as soon as room is provided for its urgent needs), and the Fish Commission with its annual appropriation of \$277,000—all of which are entirely independent of the Smithsonian Institution and of each other. Then there is the National Museum with its annual expenditure of \$175,000, and the Bureau of Ethnology with its annual allowance of \$40,000, which are still under the direction of the Smithsonian. The question is sometimes asked, and it is a proper one for scientific men to ask, whether the time has not or soon will come when these last institutions should also be put on an independent footing, and the officers of the Smith-

sonian be allowed to turn their undivided attention to other channels of usefulness? This is not the place for an attempt to answer it; but in considering this matter it should be constantly borne in mind that it is not to be settled on the abstract principle of giving to each branch of science a certain proportion of aid from the Smithsonian funds; we must also bear in mind certain points of expediency with which only those actually engaged in the management of these great trusts can be thoroughly familiar. In his last annual report, the Secretary remarks that the Institution has placed in the Museum property acquired out of its private fund (and to which it has apparently the same title) which is probably now more than equal in value to the whole of the Smithsonian bequest—that is to say, more than half a million dollars; so that even on mere pecuniary grounds it has much interest in the way that these things are cared for and used; while, on the other hand, it is clearly to the interest of the Museum to retain its present relations to the Smithsonian Institution in order to secure funds and the permanency of its staff.

It should also be remembered by those who think that it is time that the physical and chemical sciences should receive more stimulus and aid from this source, that for the last twenty years it has been able to do little more in the higher branches of biology than it has done for what they call the exact sciences. The work of exploring, and of collecting, classifying, naming and displaying specimens in a natural history museum, relates almost entirely to morphology—to the shape, size, arrangement, etc., of organs or parts; but the chief interest of these from a scientific point of view is in their relation to function—that is, to physiology. Do not understand me in saying this as depreciating the value and importance of morphological studies; on the contrary, they are one of the essential foundations of scientific biology, just as meteorological records are essential for a science of meteorology; but the scientific interest of the structure of an animal lies mainly in its relations to the functions of the living organism, and the data of structure must be combined with those obtained from vital chemistry and experimental physiology to obtain the higher and broader generalizations of scientific biology. The most minute dissections, the most careful study of thin sections of dead embryos, animals or tissues, can never take the place of direct experimental investigation of the actions of the living mechanism under varied conditions of environ-

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ment, and it is for such investigation that there is now the most urgent demand, and the best reasons to hope for important results, from the point of view of pure science as well as of practical utility. Just at present, in this country, embryological and morphological research is fairly well provided for; but physiological chemistry is not, and it is precisely in this direction that there is the greatest need of original work to determine some of the many unknown relations between structure and function in so-called vital processes, and thus to indicate new problems and new methods of research in both physiological and pathological biology.

The main points which require consideration at present are that the Smithsonian Institution has almost no funds available for scientific research; that it is highly desirable that it should have a special fund set apart for experimental research in chemistry, physics, and biology, and that if the National Academy sets to work in earnest to see that such a fund, yielding about \$20,000 per annum for the purposes indicated, is provided by the General Government as a permanent addition to the Smithsonian fund, there is good reason to believe that its efforts will be crowned with success, and that the result will be to greatly expand the sphere of usefulness of this most important Institution for the increase and diffusion of knowledge among men.

The two men who have exerted the strongest influence upon natural history studies in this country are Louis Agassiz and Professor Baird. In many respects they were very unlike; circumstances gave them widely different fields, and they worked on different plans and by different methods. They began their public career in this country almost together; but Agassiz was already famous, as the result of seventeen years' incessant work, while Baird was an almost unknown youth. Agassiz was a born teacher, a fascinating lecturer, gifted with eloquence which won its way everywhere; Baird could only speak freely in the presence of a few, and for the most part taught only by the pen and by example. Each of them created a great museum in spite of many obstacles, the first winning the means largely from private contributions, which were a tribute to his eloquence; the second gaining his end more indirectly, through his connection with the Smithsonian Institution and the Government. Each of them gathered around him young men who were stimulated and encouraged by his example, who followed his methods, have continued his work, and have taught

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others, so that there are now observers and workers almost everywhere. The first made great use of the microscope and of embryology; the second very little, for he had to use the material available. The first had a vivid imagination which led him to frame many theories and hypotheses to be verified or disproved by future investigation and research; the second classified the facts before him, but theorized very little. Professor Baird's career as an original investigator was hampered and finally stopped by his administrative work, but in proportion as this latter increased he was able to furnish materials and opportunities for others. The pupils of Agassiz and Baird are the working naturalists of to-day and the teachers of those who are to come, and the two methods of study are being combined and developed to produce results of which we already have good reason to be proud, and the end of which no man can foresee.

Allusion has been several times made to Professor Baird's power of continuous labor, which was in fact one of the main reasons of his success. As in most similar cases, it was not merely that he had a capacity for labor, but a longing for it; an urgent necessity to be doing something during every waking moment grew upon him with years, until finally there was no other amusement possible to him, and the only relaxation was a change from one kind of work to another. There was always something going forward, and this is a large part of the whole matter. Most men work for pay, some from a sense of duty, some to deaden sorrow and bring forgetfulness, a few for pleasure, and of these last was Professor Baird. This labor was carefully systematized, such a thing was done at such an hour, and there were never any vacant quarters of an hour; and every mechanical device for saving time, such as stenographers, the telephone, etc., was freely employed. He centralized all administration in himself, supervised all correspondence, and attended to small details. The wisdom of continuing to do this as the work expanded is more than questionable, for it was unnecessary waste of force; but to a man of his somewhat peculiar nature, having at the same time a keen sense of responsibility for the proper expenditure of the funds which his influence had obtained, it was probably impossible to do otherwise with comfort.

His success as a collector, organizer, and administrator was due to concentration of purpose, perseverance, tact, and constant labor. He had a clear, definite idea of what he wanted, and he did not

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scatter his energies. He did not meddle with other people's business, and thus avoided one great source of hostility; and unless a matter was in some way actually or prospectively connected with the subjects in which he was interested he had no time to give to it. He gave advice when it was asked for by any of the executive officers or legislators of the Government, and it was asked for much oftener than any one knows; but he rarely volunteered it.

He had great opportunities, but he made most of them for himself, and made the most of them when obtained.

Upon the roll of the illustrious dead of the National Academy of Sciences his name stands out as that of a scientific man of high attainments, uniform purpose, and indomitable energy, whose work has already added to the comfort and pleasure of hundreds of thousands of his fellow-men, and which bids fair to be a most important factor in supplying the necessities of millions yet unborn.

MEMOIR OF ASA GRAY.

1810—1888.

BY

W. G. FARLOW.

READ BEFORE THE NATIONAL ACADEMY, APRIL 17, 1889.



BIOGRAPHICAL MEMOIR OF ASA GRAY.

Within the short space of five years the Academy has lost its three most distinguished botanists. GEORGE ENGELMANN, a German by birth, but closely identified with American science for fifty years, died in 1884; in 1886, EDWARD TUCKERMAN, the lichenologist of North America, passed away after a brief illness, and in the year just passed we have lost him whose years and learning placed him at the head of American botanists and whose name was known and respected by the scientific men of all countries. From 1833, when his first scientific paper was published, until his death, January 30, 1888, ASA GRAY was a prolific writer and an active teacher and editor, and there have been few American naturalists who were so well known or so much beloved as he was. In him an extraordinary industry and great intellectual powers were united with a vivacity and amiability which not only made him a leader among scientific men but also brought him into intimate friendly relations with a large circle who regard his death as a personal affliction.

We need not dwell upon the details of his simple and uniformly happy life, except so far as they show the source of his influence on the progress of botany in the United States and illustrate a striking phase of American scientific education in the early part of the present century, when the young naturalist was forced to depend upon his own intelligence and enthusiasm without the aid of well-equipped laboratories and large libraries and collections.

Asa Gray was born on November 18, 1810, in Sauquoit Valley, in the township of Paris, Oneida county, N. Y. His ancestors were of Scotch-Irish descent who had emigrated to Sauquoit from Massachusetts and Vermont. He was the oldest of the eight children of Moses Wiley Gray and his wife, Roxana Howard Gray. In 1810 Sauquoit was a frontier settlement, and Moses Gray combined the occupation of farmer with that of tanner. His children, who received their early instruction at home and in the small school at Sauquoit, had none of the advantages which can now be obtained in the schools even of the smaller towns. Books were scarce, but, if few, were substantial and solid, and were read with an avidity

not often found in this age of many books. Asa was a bright boy, always ready to help his father in his various duties and quick at learning the common branches taught at school; but there is nothing to show that he was precocious, or that, as a boy, he was more interested in plants than in the other natural objects with which his life in a sparsely settled region brought him in daily contact. When about twelve years old he was sent to a grammar school at Clinton, N. Y., and later, in 1825, to the Fairfield Academy, where he remained four years. There he pursued elementary mathematical and classical studies, but they must have been decidedly elementary, for apparently Greek was not included in the curriculum, or, if so, it was of the most rudimentary kind. Gray's interest in natural science was first excited while listening to the lectures of Dr. James Hadley, Professor of Materia Medica and Chemistry in the Fairfield Medical School, who also gave some instruction in the Academy. At first, he was more interested in minerals than in plants, and it was not until later, on reading the article on Botany in Brewster's Edinburgh Encyclopædia, that his taste for botany was developed. In the following spring he made out the name of the common *Claytonia* with the aid of Eaton's Manual, and, encouraged by this, his first success, he soon became an enthusiastic collector and student of the plants which could be found near Fairfield and Sauquoit, and the long summer vacations were utilized in making botanical and mineralogical excursions to the other parts of New York State.

Moses Gray seems to have had by this time a suspicion that his son's fondness for natural history would unfit him for the work of a practical farmer, and, in 1829, he sent him to the Fairfield Medical School. While still a student in the Academy Gray had attended some of the lectures of the Medical School, and during the intermissions of the sessions of the Medical School he continued his studies with different physicians living near Sauquoit, of whom the one from whom he derived the most profit was Dr. John F. Trowbridge, of Bridgewater. In February, 1831, he received his doctor's degree at Fairfield. Gray really never practised medicine although he opened an office, which formality marked the beginning and the end of his medical career, as it has those of a good many other medical graduates. The title of Doctor clung to him, however, and he used to relate how, many years later, when traveling in Egypt, he was forced by the natives, who heard him called Doctor by his traveling companions, to prescribe for their various ail-

ments. He certainly looked like a respectable doctor, and by the aid of a few old bottles and labels he accomplished wonderful ophthalmic cures, and was even obliged to witness the somewhat embarrassing gratitude of a poor father whose paralytic son he had cured by a judicious use of the English language.

Medical studies at that date could not have been wholly absorbing, for Gray managed, while a medical student, to collect a good-sized herbarium, to say nothing of minerals, and began a correspondence with Dr. Lewis C. Beck, of Albany, and Dr. John Torrey, of New York, which served to introduce him to the best botanical circles of the country. After graduating he became instructor in chemistry, mineralogy, and botany in Bartlett's High School, at Utica, New York, and in June, 1832, he gave his first course of public lectures on botany at the Fairfield Medical School. A little later he gave a course on mineralogy and botany at Hamilton College. With the money earned from these courses he was enabled to make botanical excursions to Niagara Falls and other parts of New York and New Jersey.

The turning point in Gray's career which made it plain to himself that he could and should devote himself to botany was the call to New York, in the autumn of 1833, to be the assistant of Professor Torrey. To be sure, Torrey was professor of chemistry rather than botany, and Gray was obliged to assist in the chemical laboratory, but the real interest of the two centered in botany, and while in New York Gray published some original papers, of which the *Monograph of the North American Species of Rhynchospora* (1835) was his first purely botanical paper, although he had in the preceding year issued the first part of a series of dried specimens of North American *Gramineæ* and *Cyperaceæ*, and in connection with Dr. J. B. Crawe had published a *Sketch of the Mineralogy of a Portion of Jefferson and St. Lawrence Counties, N. Y.*, in the American Journal of Science. Owing to lack of money he could not be employed as Professor Torrey's assistant after 1833, but he returned to New York in 1835 and became Curator and Librarian of the Lyceum of Natural History, and while thus occupied prepared in leisure moments his first text-book, *Elements of Botany*, which appeared in 1836. He was hard at work with Professor Torrey on the *Flora of North America*, during the summer of 1836, when he was appointed botanist of the Wilkes Exploring Expedition. But as the sailing of the expedition was, for one reason or another, post-

poned for two years, Gray, becoming weary of the many changes and delays, resigned his position. At this time a new State University had been founded in Michigan, and Gray accepted the chair of botany which was offered to him, on condition that he should be allowed to spend a year abroad in study before beginning his duties. He returned to America in November, 1839, but he never entered upon the duties of his professorship, for, owing to financial or legislative embarrassments, the services of the botanist were not at once needed in Michigan, and Gray therefore remained in New York at work on the *Flora*, of which the first volume had appeared (1838-1840), and the second, then well under way, was completed in 1843.

During a short visit to Boston and Cambridge Gray had made a favorable impression on President Quincy, of Harvard College, and soon after his return to New York he was offered the newly endowed Fisher Professorship of Natural History at Harvard, which he accepted. He removed to Cambridge in 1842 and entered upon his duties. On May 4, 1848, he was married to Miss Jane Lathrop Loring, daughter of Mr. Charles G. Loring, of Boston, who still survives. From this time his life ran smoothly on. He was now able to devote himself wholly to botany and the building up of the botanical establishment at Harvard University, which he may be said, in fact, to have created. He brought together books and plants and gathered round himself numerous students, young and old, who wished to profit by his knowledge and the botanical treasures which he had accumulated. From Cambridge his influence extended in ever-widening circles until, as time went on, he became the one man to whom American botanists looked for advice, sympathy, and inspiration. With age came respect and honors from home and from abroad, and even time dealt kindly with him, for he was still as fresh, as active, as cheerful as ever when he returned from his last European journey, in October, 1887, and turned with renewed zeal to his work on the *Flora*, which he was destined never to complete, for, on the morning of November 28, he was stricken with paralysis and lingered in a partially conscious condition until the evening of January 30.

The life of Asa Gray fills some of the brightest pages in the history of American science. He occupied a place in the public esteem which no other botanist had ever held, and his death leaves a gap which cannot be filled. It is one of those rare cases where the right man appeared at the right moment. There had been many and

good botanists in America before his day, but they were forced from their own necessities and the state of science in their time to do the rougher work of exploration, or, if they attempted other work, to do it at a disadvantage. Some were poor and wandered from place to place, living as best they could. Others could only devote leisure moments to botanical studies after their best hours had been spent in business or the drudgery of teaching. Even the most favored worked at a great disadvantage, in the absence of large herbaria and special libraries. Their work was perhaps the best that could have been expected under the circumstances, and although its value was fully appreciated by the small botanical public, it was too scattered and fragmentary to reach the larger public and make itself felt on the large body of those who, although they recognized the value of physics and chemistry, had not yet learned that botany, too, was a science and not merely the amusement of a few mild-minded, odd-looking people. Botanists themselves needed some one who could bring together the scattered materials of the early explorers and harmonize the writings of earlier botanists into a compact and comprehensive whole; one who could settle authoritatively doubtful points of nomenclature; who could describe species tersely and clearly so that there might be a good general account of the flora of North America comparable with similar floras of Europe. The public needed some one to tell them what botany itself was and what botanists were doing. Evidently he must be a rare man who could hope successfully to perform two so widely different tasks. The works of Pursh, Michaux, Nuttall, and others had made an admirable beginning, and had Torrey not been forced to live by his chemistry, he might, perhaps, have accomplished the former task alone. Or what might not have been expected of Engelmann, thoroughly trained in the best European schools, had he been free from the demands of an active professional life? But neither Torrey nor Engelmann, in spite of their scientific attainments, could have accomplished the second task, for which Gray was peculiarly fitted. Combining the power of original research with a talent for popular exposition, he was just the man for the time. Younger than his friend and teacher, Torrey, by fourteen years, he began his botanical studies at a fortunate period. The artificial system of Linnæus was beginning to yield to the natural system, and Gray began with the new school of systematists who were destined to carry all before them, and he lived to be a prominent

defender of the still newer school of Darwin, which sees in species not fixed creations but stepping-stones in a grand progress from below upwards. But we should not suppose that in 1836 Gray accepted the natural system merely perfunctorily. At that time it required some courage to advocate a system to which most American botanists were opposed, just as in 1859 it required courage to defend a theory regarded by many naturalists, as well as by the general public, as atheistic. In his *Elements of Botany* he had to defend the natural system, and while most of the notices of that book were commendatory they were also largely apologetic.

At the outset of his career Gray recognized that the first step in the development of the botany of any country must be the description and classification of its phænogamous species. That step once taken the way is open to studies of another kind. He turned his whole energy in this direction and did his best to accomplish the task, and, although death interrupted him in the midst of his labors, the work was so far advanced that it can be completed by his fellow-worker, Sereno Watson. The first conception of the *Flora* originated with Torrey, and under his able and friendly guidance Gray made rapid progress and soon rivalled his master as a systematic writer. But neither Torrey nor Gray had any real conception of the magnitude of the task they had undertaken. Material accumulated so rapidly that before the two volumes of the original *Flora* were finished it was evident that they must at once be revised. In his first visit to Europe, November, 1838, to November, 1839, Gray visited all the leading herbaria of Europe, from Padua to Berlin and Glasgow, examining carefully the types of North American species, and became personally acquainted with several of the early explorers, as Menzies and Auguste Saint-Hilaire. He was everywhere favorably received and his agreeable manners and botanical enthusiasm attracted the attention of the leading botanists, such as R. Brown, Sir W. J. Hooker, and A. P. De Candolle, and formed the basis of an enduring friendship with the younger men of promise, as A. De Candolle, Sir J. D. Hooker, and Decaisne.

The second volume of the Torrey and Gray *Flora*, ending with *Compositæ*, was published soon after Gray settled in Cambridge, but the work was never to be continued, for Gray had now formed a plan of writing a new *Flora* on a more extended scale. At Harvard he could devote most of his time to the work, for his professorial duties were not, at first, very onerous. But there was practically no

botanical library or herbarium, except what he had brought with him to his new home. The Botanic Garden in Cambridge was founded in 1805, and W. D. Peck was director and professor of botany until his death, in 1822. The endowment was then so small that a professor could not be appointed, but Thomas Nuttall was made Curator of the garden and afterwards Lecturer on Botany. He resigned in 1833, dissatisfied with his position, and the garden was then left in charge of a gardener until the arrival of Gray, in 1842. Before good work could be done it was necessary to have books and specimens, and Gray set to work to collect these. A salary of \$1,000, which was all that he received for some years, would not now seem to us to afford much opportunity for purchasing books and plants. But Gray was trained to be frugal, and he soon managed on this scanty income, with absolutely no property of his own, to bring together a large library and to create an admirable herbarium. The small house in the Garden in which he lived at last became so crammed with books and plants that it might almost be said that guests had to sleep on plants, and that the china closet had become a library. In 1864, dreading the danger from fire, he offered to present his collections to the college on condition that a suitable building should be erected for their reception. His offer was accepted and a small brick building was built connecting with his house, and afterwards a small library was added. In 1865, when presented to the college, it was estimated that the herbarium contained at least 200,000 specimens and the library about 2,200 works, not including pamphlets. Their money value is well up in the thousands, and in these days, when the names of those who have given a tithe of their wealth to educational institutions are loudly proclaimed in the papers, we may well ask that the name of him who by his frugality and industry was able to give so large a sum to his adopted alma mater should appear prominently in the roll of her benefactors. At the time of Gray's death the herbarium was probably double the size that it was in 1865, and it is now by far the largest and most valuable herbarium in America, excelled by but few of the older herbaria of Europe, and has been the resort of nearly all the botanists who have within the last thirty years written concerning North American Phanogams.

The greater part of Gray's scientific work during the thirty-five years following the completion of the second volume of Torrey and

Gray's *Flora* had a more or less direct bearing on the contemplated revision and enlargement of that work. An immense number of monographs of different genera and orders, reports on the collections of different expeditions, notes on structure and nomenclature were published in the journals and proceedings of societies in this country and abroad, and at length, after this elaborate preparation, the first part of the *Synoptical Flora*, including the *Gamopetalæ* after *Compositæ*, appeared in 1878. A second part, including from *Caprifoliaceæ* through *Compositæ*, appeared in 1884. In 1886 supplements to both parts were issued, and he was at work on the *Polypetalæ* and had nearly finished the *Vitaceæ* when attacked by his last illness.

Gray was pre-eminently a systematist, not because he underestimated the value of other work, for he encouraged younger men to devote themselves to other departments of botany and was always interested in their success, but rather because systematic work was what was most needed in his day and that for which he himself was best fitted. He did well to limit himself to this field, where he felt sure of his ground. His quick eye, which enabled him to catch the essential features of any plant at a glance; his clear judgment, which distinguished between what was constant and typical and what was accidental and variable; his remarkable memory, his clear, terse style, placed him at the head of American systematists and made him rank with the great botanists of the world. He has been compared with the elder De Candolle, whom he certainly much resembled in the method of his work and his manner of writing. He had a wider knowledge of North American Phanogams than any other botanist, and in his knowledge of the *Compositæ*, the largest order of flowering plants, he was unequalled.

But Gray was more than a systematist. His mind looked beyond mere classification and description and sought to solve the perplexing question of plant-distribution. As early as 1840, if we may judge from a review of Siebold's *Flora Japonica* in the *American Journal of Science*, he was struck with the resemblance of the flora of Japan to that of Eastern North America. His first paper on plant-distribution (1856), entitled *Statistics of the Flora of the Northern United States*, was written partly in response to a request from Darwin for a list of American alpine plants. Here he showed by means of tables the comparative distribution of species in Europe, Asia, and North America, and announced the fact that the

resemblance of our flora to that of Europe is due not to the large proportion of genera common to the two continents, but rather to the number of identical or closely related species, while, on the other hand, the number of characteristic genera common to the Eastern United States and Eastern temperate Asia is remarkable, and a very ancient dispersion was suggested to account for some anomalous cases. It was, however, a study of the Japanese plants collected by Charles Wright, the botanist of the North Pacific Expedition of Ringgold and Rodgers, which enabled Gray to arrive at the far-reaching results on which his fame as a philosophical naturalist principally rests. His paper *On the Botany of Japan and its Relations to that of North America and of other Parts of the Northern Temperate Zone* was a masterpiece, in which he showed the relations of the floras of the North Temperate regions, accounting for the present distribution by migration of species from the Arctic regions due principally to the different climatic conditions of the pre-glacial, glacial, and post-glacial eras.

To understand the gradual change in Gray's views as to the nature of species it is only necessary to compare his review of the *Vestiges of Creation*, written when he was 36, with the paper on the *Botany of Japan*, to which we have just referred, written when he was 12 years older. The review is very interesting and suggestive, but there is in it something of impetuosity and assertiveness which suggests immaturity. It represents the views of what the writer thinks, on general grounds, ought to be true. The latter paper states what, after careful inquiry, he has found to be true. In 1846 he believed firmly that species "were created as perfect as they now are." After the lapse of twelve years he had become convinced that the present species are not special creations, but modified forms derived from pre-glacial ancestors. His own special studies having brought him to this conclusion, it was natural that he should look with favor upon Darwin's *Origin of Species* when it appeared, in 1859. In fact, Darwin's views were already known to Gray, for the two had been in intimate correspondence since 1855, and Gray was one of the three persons to whom advance sheets of the *Origin of Species* had been sent before its publication. To Gray there was nothing alarming or irreligious in the theory of evolution. Although a devoted member of the Congregational Church he was not frightened by the general cry of atheism with which the new book was greeted. He showed a truly scientific spirit in his discussion of the

question and was considered by Darwin himself to be the best expounder of his views. His presentation of Darwin's views was, in fact, so clear and open that it has often been said that Gray accepted Darwinism to its full extent. But this is a mistake. He was indeed candid in representing Darwin's views in such an attractive and impartial manner as to disarm those who attacked them as irreligious and opposed to design on the part of a Creator, but he never professed to follow Darwin beyond a certain point. Gray considered that natural selection is, on the whole, a good working hypothesis, but cannot explain how wholly new parts are initiated, even if the new organs are developed little by little. He denied that natural selection accounts for variation. The power to vary once given, then natural selection will account for the present forms of life. But whence comes the capacity for variation? Gray differed with Darwin in believing that variation occurs in certain more or less regular directions, and particularly in beneficial directions, by virtue of some inherent power imparted in the beginning by Divine agency. But this is a restatement of the problem in another form rather than a scientific explanation. Theology may speak of an inherent power, but Science will still ask, What is that power? Again, what is meant by beneficial? The word is ambiguous, for, since only those variations which tend to enable living beings to survive in the struggle for existence can, in the long run, be called beneficial, if we assert that variation acts generally in beneficial directions we are the same as saying that variations generally tend to enable existing beings to survive, which is by no means evident. But, whether we adopt Gray's modification of Darwinism or not, we must admire his impartial statement of Darwin's case at a time when many of our leading naturalists treated the question with scorn, if they did not go farther and indulge in abusive remarks concerning one now ranked among the bright lights of science.

While Gray's purely scientific writings gave him a high place among botanists he exercised a powerful influence in a larger sphere through his work as a teacher and writer of educational works and through his numerous papers of a popular character. In the classroom he came in contact with a large number of students, now scattered in different parts of the country, who recall with pleasure the genial instructor so absorbed in his subject that he assumed that every one else must be equally interested in it. As an extempo-

aneous lecturer Gray was not especially fluent, but there was an eloquence in his simplicity and earnestness. Upon the more advanced students who at different times worked under his direction in the herbarium he made a deep impression. They were treated rather like younger friends than students. The first thing was to work and work conscientiously, but solemnity and the appearance of excessive wisdom were discouraged. One was taught to work with a will and for the love of botany itself, not for the sake of making a display of learning. He detested shams and nothing pleased him more than to prick the bubble of pomposity or, by some well-timed remark, to check the stream of those who talked for the sake of talking. Once, on listening to a lecturer who had the habit of saying very little in a great many words, he said to the writer: "Don't ever lecture *about* botany. When you lecture, say something." Vagueness was also discouraged. The student had it impressed upon him that there should be some definite point to his work; that the end of scientific activity should not be an indefinite general culture.

The numerous editions of Gray's *Text-Book* made his name known in all the higher schools of the country, and the *Manual of the Botany of the Northern United States*—a work which may be enlarged by future discoveries, but whose general character could hardly be improved—made it possible for any one to gain an accurate systematic knowledge of the flowering plants of the region, while the attractive little books, *How Plants Grow* and *How Plants Behave*, were the means of inducing many to enter upon a study which they afterwards pursued with pleasure and profit. By these works, as well as by his papers on Darwinism, Gray became not only a recognized scientific authority, but his opinion on all sorts of topics, religious, philosophical, etc., was sought by the journals of the day, and such was his fertility and good nature that he generally wrote articles at their request even against the wishes of some of his scientific friends.

In his descriptive works Gray's style is marked by clearness and a singularly happy use of short, expressive sentences, and his general and popular essays are written in a style which would do credit to any cultured literary man. There is none of the crudeness, the slovenliness, or the awkwardness which too often mar the writings of scientific men who, however learned they may be in their own department, are frequently either unable or unwilling to take the

trouble to express themselves clearly and in correct language. The excellence of Gray's style as well as his great fertility is well seen in the immense number of reviews and notices in the *American Journal of Science*, with which he was connected as editor for more than thirty years, and in his numerous papers on Darwinism. The sentences flow smoothly and gracefully along, relieved by numerous apt similes and brightened by sparkles of delicious humor. The number and variety of the reviews are something extraordinary. He had a passion for reviewing, and no matter how fatigued he might be with the day's work he seemed always to be ready to refresh himself in the evening with another review. As a critic of other men's work he was discriminating without being ill-natured, and it is probable that few persons could have written so many reviews without making for themselves a host of enemies. But Gray was on good terms with nearly all the world. In fact, it was to his own happy disposition that his success was in part due. Had he been morose and repelled his contemporaries it might have been left to a future generation by digging and delving to ascertain his greatness as a botanist. But he was of an even, cheerful disposition, and believed in being happy himself and in making those about him happy. With a kind heart, a clear head, and a vigorous body he was convivial as well as learned, and was as much a man of the world as one who loves his science can afford to be. He was at home in any society, no matter how distinguished, always entertaining and not merely entertained; self-possessed without being self-conscious. Not a distinguisher of persons, but uniformly kind and sincere, it is no wonder that he was beloved wherever he was known.

But I need not recall to you, members of the Academy, his attractive ways. His face was long familiar to you. He was often seen here either in attendance on the sessions of the Academy or in discharge of his duties as a member of the Board of Regents of the Smithsonian Institution, and you have often met him at scientific gatherings elsewhere. You know that he was always quick-witted, running over with anecdotes, and always eager to hear what was going on in the scientific world. And some of you will recall hours spent in his quiet home at Cambridge among his books and flowers, his only children. His whole life was as beautiful as it was fortunate. He found a country in which a few botanists were struggling against general neglect and popular ignorance. He left a great nation in which, very largely through his exertions, the value of

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botany had become generally recognized and in which a crowd of young workers had arisen anxious to carry out the good work even in the most remote regions. But the times have changed and the range of botany has become so wide that, although we may have good botanists in the future as well as in the past, it is hardly likely that hereafter any single botanist will hold the same comparative place in public esteem as that filled by Asa Gray in his day.

A list of the writings of Prof. GRAY will be found in the appendix to Vol. XXXVI of the American Journal of Science for 1888.

MEMOIR
OF
JOHN CALL DALTON.
1825-1889.

BY
S. WEIR MITCHELL.

READ BEFORE THE NATIONAL ACADEMY, APRIL 16, 1890.

BIOGRAPHICAL MEMOIR OF JOHN CALL DALTON.

GENTLEMEN: Our late member, JOHN CALL DALTON, was for years a peculiar and influential personage in the profession of Medicine in America. There had been before his day men who were noted for single papers on physiological subjects or who had spent brief periods on physiological investigation. Some of the work thus done was valuable and original, but always it was that of physicians who stepped aside from their other pursuits to follow experimental study for a brief season, and who went back again to the ordinary routine of their profession. It would, indeed, be too easy to enumerate their essays.

The man to whose memory I devote these pages was the first among us to give a life to the single pursuit of experimental physiology and its related sciences. I shall presently speak of what he was and did, but it is humanly interesting to note here that out of this singleness of pursuit and out of the fact that he was the first to content himself with physiology as a laboratory life-work there came to him a reputation above and beyond that which his discoveries justified, but which was fairly the reward of an unselfish devotion, which followed with simplicity of purpose and indifference to material gains one single path untroubled by greed and undisturbed by mean ambitions.

He was our first professional physiologist. Of no other science save physiology could it be said that it had then no practical students in America. Before his day there had been esteemed professors who, like Robley, Dunglison, and Samuel Jackson, possessed singular powers of acquisition, critical ability, and eloquent capacity to express the thoughts of others. A physiological lecture was in that day a more or less well stated resumé of the best foreign books, without experiments or striking illustrations. It was like hearing about a foreign land into which we were forbidden to enter.

I well recall the really admirable lectures of my own preceptor, Dunglison, who was brought to America by Thomas Jefferson to lecture at the University of Virginia, and thence came north to the school at which I learned as much of my profession as men learn

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on college benches. I am amused to remember how much of the physiology I learned seemed to have but two dimensions. The imagination was freely called upon to aid us in our conception of active functions. We saw, for example, as illustrative of the circulation of the blood, certain diagrams, and also a dead bullock's heart layed open to exhibit the cavities and the valves. Years afterwards I first saw in an etherized animal the living, moving heart. The swift certainty of the successive motions of this bounding thing (itself like an individual existence) filled me at once with a fresh conception of the delicacy and wonder of the vital mechanism amidst which I had been moving, so to speak, with but the slightest realization of its marvel and mystery. Now, at last,

"I saw with heart serene
The very pulse of the machine."

The vehement energy of movement—certain, accurate almost as a clock—filled me then for a time, as it did the great Harvey, with a sense that surely it was to be comprehended but by God alone. I seemed to have become before this coarsely familiar with the human body—familiar, perhaps, to disrespect. A single object-lesson on the living heart, a thing unseen by me before or only heard or felt, left me with a never lost and most useful respect for the mystery of it all, and a feeling of need for care which could not be too thoughtful.

Since then, as a teacher, I have seen the good influence of such object-lessons on other men who were learning the sadly uncertain Art of Medicine in youthful disbelief of uncertainties.

In my student days, as I have said, no teacher habitually brought us face to face with these phenomena of vital physics, and when once they were shown their influence on intelligence was lasting. Nature is, alas, only too shy; but we may at least get nearer to what she reveals than a mere description. For the neglect of the classical scholar-professor to show, as far as might be, the animal mechanisms in action there was reasonable excuse. No man is justified in showing to a class mere repetitions of experiments at the cost of pain to animals, but about the time Dalton began to teach ether enabled us to do without pain what before would have been cruel. He was prompt to see that a new era had opened for the teacher of physiology.

Dalton began almost at once to illustrate with living animals the processes of life as he taught them in lectures. He was the first to

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do this in America. It was a vastly useful lesson to all who followed him, for lectures should be largely commentaries on experiments; almost all else can be had in books. But books do not *show* arterial pressures; they only *state* them. They but *describe* the heart, whose perplexity and mystery of motion must be *seen* to be appreciated. I confess to the belief that in medical teaching most didactic lectures might be dispensed with and books and recitations take their place. But in Physiology, Anatomy, Therapeutics, and Chemistry the lecture with its profusion of illustrative experiments should remain.

As his practical skill in experiment led Dalton to use it in adorning his teaching, it tempted him very early into the field of research, and here for many years my friend and I were quite alone—the pioneers among us of work now become large and productive, but then looked upon with doubt and even suspicion, as something with which the practical doctor had better have no concern. Dalton, unembarrassed by daily practice, went his way untroubled and was, I believe, the first American Physiologist who declined to practice medicine. Older men not only disapproved of this divorce, but also considered the active pursuit of physiological knowledge in the laboratory as inimical at least to worldly success in the practice of medicine. Prof. Samuel Jackson once said to me, "You will lose a patient for every experiment you make in the laboratory." But times have changed, and he was altogether wrong.

John Call Dalton was born in Chelmsford, Mass., February 2, 1825, of a line of able physicians, himself destined to give the name a permanent place in American Science. Doubly indebted to Harvard for degrees in arts and medicine, he found in Paris a teacher under whom he turned with eager zest to experimental physiology. I remember having heard Dalton, when praising his great master, tell how once he himself had said, "I think it must be so;" to which Claude Bernard replied, "Think! Why think, when you can experiment?" Bernard strongly influenced the men who sought his courses, and I for one, like Dalton, must gladly acknowledge the educative powers of this sturdy genius.

On his return home Dalton calmly put aside the ambitious calls of mere practical life in my profession; nor, indeed, did he seem to me fitted to excel in these. In 1831 his essay on the "*Corpus Luteum*" received a prize from the American Medical Association; nevertheless, in 1860 he advised me against contending for the same

prize. I do not now recall why. I sent in as an essay my well-known Smithsonian work on "Serpent Venoms," and was not the winner of the prize.

Soon came the appointment to teach Physiology at Buffalo, which he left for the same work in 1854 at the University of Vermont. From this Chair to the Long Island Hospital School we trace him in 1859, and this year also he finished his Text Book of Physiology, which in 1882 had reached its seventh edition—a clear, concise treatise, in that day without rival, for it was written by one who had seen experimentally almost every known fact of the science he taught. Then came a long break in the scientific record.

The War made a claim upon Dalton to which he replied without hesitation. Amidst the numerous demands made by that great struggle none were so hard to fill as those for the best surgical advice. The history of what my profession did in and for the great cause has yet to be fitly told. I think Dalton little liked the life in the field and hospital, and this is shown by the fact that with all his great powers as an intellectual observer his experience of thousands of cases evolved nothing of value. He had not the type of mind needed for clinical study. He was at this time Professor of Physiology and Microscopical Anatomy in the College of Physicians and Surgeons in New York, and urged me to assume his lectures while he was absent; but this my own hospital duties rendered impossible.

Soon after the War he became full Professor of Physiology in the great New York School and declined all temptations to go elsewhere—lecturing, working, observing, experimenting, and only tempted forth from the laboratory to take the place of Vice-President of the New York Academy of Medicine from 1874 to 1877.

During many years the public knew little of Dalton, except when he appeared before legislative committees or in print in defense of Science against Mr. Berg and such others as wished to carry legislative interference even beyond the follies of the British law, and to deny to us all use of animals in the laboratory. In this contest Dalton was always successful. His intense, fiery spirit, his profound convictions and vast knowledge, routed all opponents. His singular ability in the management of his case was not less remarkable than his skill at logical defense or his wealth of defensive illustration. For this fierce and able battle we owe him much, and there will be little need to re-argue the matter whilst his presenta-

tion of it remains in his delightful little volume on the "Experimental Method in Medicine" and his "History of the Doctrines of the Circulation." In the former book is an essay on Buffon's "Theory of Organic Molecules" and Buffon's "Theory of the Inclusion of Germs." I know few essays on dead and very dry science made so interesting as this by the mere literary qualities of the writer—a matter too much neglected by men of science; for assuredly it is true that even an arid description of a shell or a beetle may gain interest from the charm of a style precisely fitted to the task. The man is not always to be found between the pages of his book, but in these essays we get some glimpses of a man keen and logical, faintly cynical in no ill-tempered fashion, and here and there unexpectedly humorous, as he was in social life.

Of a quantity of papers on the cerebellum, placenta, digestion, hydatids, toenia, etc., I have not time to speak in full. One of his best pieces of physiological work was a brilliant experimental essay on the "Sugar-making Functions of the Liver." After Bernard's revolutionary discovery of this new liver function it was denied by Pavy, who really seemed to have demonstrated the great Frenchman's error. Dalton's paper brushed away the whole critical fabric of Bernard's opponents, and since then physiological opinion has remained settled upon this question.

Of late years the Presidency of the College of Physicians and Surgeons, keen contests with the vivisectionists, and alas, too, increasing ill health and its sad claims for rest combined to lessen and at last to stop original work. He complained that societies, affairs, and public trusts took up his time, and this was true; for when the public find a man like Dalton, honorable and energetically efficient, it is prompt to weight him heavily, and this despite the belief that the man of science is inapt for business; whereas he is competent as few men are when he chooses to drop his higher purposes and exercise his mind with the mere routine of business life or with the administration of affairs involving method and common sense.

In 1883 he thought best to resign the Chair of Physiology, but during the six remaining years of his life was as active as increasing disease permitted in his post of President of the College of Physicians and Surgeons. His later years were occupied with his great morphological work on the "Topography of the Brain." For this he personally prepared all the specimens, and with sedulous care

followed the photographs made in his presence through the processes which at last ended in their production in his book. Of these noble quartos of superb illustration and careful description one may frankly speak in terms of warm praise. I know of no other "Anatomy of the Brain" so satisfactory for reference. It is pure gross anatomy of surface or sections. No word of tracks or fibres or of functions; it is all pure macroscopy.

John Dalton was elected a member of the Academy in 1864, and for a long time was seen at our meetings; but very far back he began to feel the lack of energy due to the onset of disease, and ceased to be with us often. Historically, he represents in our story the first academic recognition of Physiology. Few of us, perhaps, recall his quaint, earnest, attentive face, accentuated as to feature, bearded to the loss of facial expression; the forward set brow, the keen eyes, at times vividly humorous in expression; something about his face very honest, very intent, very strong—a face which surprised one by its sudden illuminations.

He had, says a pupil, the "teaching instinct"—thinking of class, of theme, of the questioning faces before him, but never of himself or how he would appear. No side issue disturbed him, and he had the skill to make the difficult appear simple, and evidently rejoiced in his power to make things plain. Now and then some side light gave a glimpse of his encyclopædic knowledge, but he made no excursions for mere amusement's sake; all was clear, logical, and the manner of it earnest, yet lively. At times his illustrations were the inspirations of the moment, as when to show the relations of the corona radiata in the brain to the basal ganglia he began to arrange hastily scraps of felt on the radiating fibres of a broom. He had the rare gift of making those who listened desire to become investigators. He made men think. Possibly lecture interests absorbed too much of him, but he really created a method of teaching his branch such as was before unknown among us.

Dalton was a man of whom I saw much at times. He once came to me to offer a Chair in the College of Physicians and Surgeons, and once I went to him with the gift of the Chair of Physiology in the University of Pennsylvania. He frequently consulted me as to his own work, and he took in mine the warmest and most unselfish interest, and was more pleased than I when something new and valuable was uncovered.

In social life he was inclined to be quiet; but if disturbed as to

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his political or economic views (which he held with tenacity) he was an interested, swift and ready talker, defending himself with skill and humor, and at times with excess of vehemence.

It is pleasant to show another side of this vigorous personality. As a man he lived the simplest of lives—a bachelor, in a tiny suite of rooms, eating quietly at his club, unstirred by the venal ambitions of the roaring world of greedy trade around him in New York. Here among the greater books of the past, with a wide choice of subjects, you might find him with Cicero, the Arabian Nights, or Galen or Sydenham. This quiet scholar and profound man of science was tranquil and self-possessed under fire in battle, as I have been told by one who had seen him in the field. He was a bold horseman, and in his younger days a venturesome rider across country.

A long and painful illness at last made impossible the labor which he best loved, and one by one he fell out of the public and social relations which made life valuable to others and pleasant to both them and himself.

A year before he died, an attack of uræmic convulsions occurred and left him for a time with asphasia. Says a friend* "It was almost amusing to observe the characteristic curiosity with which he watched the phenomena of his disease, studying it apparently with as much interest as if he were observing it in another person."

Despite the nature of his malady, he carried his mental clearness and active intellectual interests near to the time of death, as it were, undisturbed by the crumbling ruin of his bodily frame. Well aware of the approach of death, he met his fate with quiet courage, and so came to the end of a well-filled life, in which he brought to the service of his country his science, his profession, persistent energy, great intelligence, a fine sense of honor, and all the minor characteristics of a true and manly gentleman.

* W. H. Draper, M. D.



MEMOIR
OF
LEO LESQUEREUX.

1806-1889.

BY
J. P. LESLEY.

READ BEFORE THE NATIONAL ACADEMY, APRIL 16, 1890.

BIOGRAPHICAL MEMOIR OF LEO LESQUEREUX.

LEO LESQUEREUX was born November 18, 1806, at Fleurier, Canton de Neuchâtel, Switzerland, and died at Columbus, Ohio, October 20, 1889, at the age of 83.

The following short sketch of his life and works is made partly from a series of autobiographical letters which I happily induced him to write to me between December, 1885, and the following May, embracing the first 36 years of his life, and partly from my intercourse with him during the last forty years. As I intend to give his own charmingly naïve narrative to the public, I will confine my extracts from it now to the parts of it which have a special interest for men of science—the youthful growth of his character as a student of nature and the earliest investigations into which he was led.

The Pontarlier railway route from France into Switzerland descends southward by a long ravine into the Val de Travers at Fleurier. There, turning east, it proceeds along the narrow level floor of the valley, between mountain walls a thousand feet high, and issues by one of the wildest gorges of the Jura upon the north shore of the lake, and so reaches Neuchâtel, thirty miles from Fleurier.

This was the grand and beautiful region in which the Huguenot Squirrels (*L'écureuil*) settled when driven from France by the Revocation of the Edict of Nantes. "A large number of the French families of the Cantons of Geneva, Vaud, and Neuchâtel came from France, like Agassiz and Guyot, for example—people deeply religious, of severe morality, and of simple habits; comparable, indeed, to the Pilgrims who took refuge in America." And as the English Pilgrims have made despotism impossible in the New World, so the French Pilgrims not only checked the establishment of a permanent universal despotism in Europe, but established the republic which is sooner or later to take its place everywhere.

The male inhabitants of Fleurier, like those of other villages of the Jura, were and still are industrious watchmakers; the women were lacemakers, until the Dutch women took their markets from

them. In Lesquereux's young days, in fact as late as 1872, there was not, however, in all Switzerland a single manufactory of watches—that is, any establishment where entire watches were made. Each part of a watch, even the smallest piece, was made by a different person, exclusively occupied in that particular. One made only screws, another the dial, another the spring, another the hour or minute hand. Each lived in his own house and worked at home, aided by his family, and sometimes by one or more apprentices or journeymen. From these scattered workmen the *Etablisseurs* ordered the several pieces and put them together. "Each operator," writes Lesquereux, "was his own master, paid for his work according to his skill and proficiency, so much per day, or oftener so much per piece. That was, as you see, a quiet and patriarchal time. The Americans have changed all that; and now in a single factory in Connecticut they make more watches than could be made in a whole Swiss village." Lesquereux's father made watch springs, with usually four journeymen, each performing always the same part, one cutting the thin plates into strips, another giving them their proper thickness, a third polishing, the master reserving to himself the more difficult task of tempering, coloring, and finishing the springs; and for the superiority of his springs (adds his son with pride) he was celebrated in all the Jura mountains. We will see what a rôle this paternal skillfulness came to play in the life of his more celebrated son.

The father was an uneducated man, "knowing just enough to keep his accounts, write a few short letters, and read his Bible on Sundays." The mother knew more, "read much, and had the greatest regard and concern for scientific people, especially for the preachers," and she induced the father to give his son a liberal education to become a minister. "I owe to my mother," he writes, "all the influences which were developed later in my youth. Until I left home she directed my studies at school, and after I left it I never ceased to correspond with her until her death."

But, as in many another case, the boy whom his mother dedicated to the ministry of religion was prepared and trained by her for a very different ministry, the ministry of science. "As far back as I can remember," he writes, "my greatest enjoyment as a child was in freeing myself of all my surroundings of the village and becoming associated in some way with nature, in the woods and fields, where I could be alone and ramble without constraint." All his spare

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time out of school was spent in household work or running errands ; but on Saturday afternoon and sometimes for a few hours after Sunday school he could take long rambles with one or two friends, generally climbing the gravelly slopes to the foot of the bare rocks, and then following goat tracks up the cliffs to reach some high crag from which he could look down on the valley below. "For what?" he asks. "I do not know. I was in my element like a fish in water. I sang the whole time. I gathered wild fruits, hazelnuts, or flowers for my mother. When two or three of us were together we would detach great rocks to see them vaulting down, springing across chasms, and bursting into thousands of pieces."

Recounting his famous fall he writes: "By a misstep at the very top of the mountain, opposite the village, on the edge of a vertical cliff, I fell from rock to rock and rolled to the base like a bunch of moss down the steep slope to the borders of the cultivated meadows of the valley. I was immediately taken up and carried home by some workmen who had seen the fall. My whole body was covered with wounds, some of my limbs lacerated to the bone, my scalp cut in nine places, one of my eyelids split and torn aside; but none of my bones were broken. I had lost consciousness from the first shock, and falling from rock to rock, sometimes upon trees, without resistance of my own, I escaped death as by a miracle. I remained unconscious for two weeks, but after two months I was out and had to attend school. The council of the village had the event recorded, and a flag was erected not far from the top of the mountain, as high as two hardy workmen could ascend. I was then ten years old." A few years ago when Desor took me to see Berthoud, the artist scholar of Fleurier, he pointed to "Lesquereux's cliff."

Soon after this event the boy began his Latin studies with the village parson, "a poor preacher and a very poor instructor, from whom he did not learn much." At 11 years of age he was sent to the cleverer parson of a neighboring village, Motiers, who kept a sort of boarding school. The first year, rising at 5 o'clock, he walked the two miles, sometimes in deep snow and often in darkness. The second year he became a boarder in the school. His weekly vacations were still spent in mountain climbing. When the preacher was away or too busy to give them lessons the boy haunted a large limestone cavern, opposite the village, of which he explored all the nooks and crevices and galleries, "penetrating far deeper beneath the plateau than any grown man had ever done or could have done."

The times between school hours he spent in a nest which he had devised near the top of a high tree overhanging the little river which runs down the Val de Travers past Motiers. Here he could con his lessons undisturbed, for none of his fellow-scholars would climb the tree.

Arithmetic and writing long French letters were easy and pleasant tasks. Latin and Greek were failures. He says that his memory was extremely weak and "always unreliable." At 13 he was sent to college at Neuchâtel, greatly against his will. He had no inclination to study. "I was rather a dreamer," he writes, "very timid, afraid of people," and he desired to help his father to support the family at his trade. But the mother insisted, and the result was a failure, which he describes in a piteous and amusing manner. His first examination was made with Arnold Guyot and August Agassiz, the brother of Louis, who was then a student in a German university. Of the three, Lesquereux was "the oldest and the most ignorant," and although he worked harder than any of his classmates he "never reached any kind of those honorable distinctions so highly appreciated at school." On commencement day, in the crowded church of Neuchâtel, so well-known to American tourists, he got none of the prizes, and "no other reward but some honorable memento for good behavior, and *le prix de bonnes notes*, a big psalm book, nick-named by the boys *le prix des ânes*. His shame and confusion of soul were unbounded, increased by the presence of the Prussian Governor and other high officials. Mother and son, without stopping to eat, took their melancholy way on foot back to Fleurier. Two years of toil had been wasted; time and money lost; the family disgraced. The boy could answer nothing to the reproaches. He could do nothing but weep and say: "You heard from Mr. B. that for months past I have worked at least sixteen hours a day, often leaving my studies at midnight to take them up again at 3 or 4 o'clock. I cannot learn anything. I cannot remember anything. The work is killing me, and I get nothing but unkind words from you. Why do you force me to study Latin and Greek and such stuff, worth nothing to me, while I could be so happy at work with my father? I cannot go back to Neuchâtel."

Yet, four weeks later, he did go back. His collegiate instruction was at an end; his academic life began. "This was the end of my youth and the beginning of manhood; a great change, which opened

the true spring, the most beautiful and too short season of my life." The *auditoires* did not demand long preparation. The professor read and explained the classic authors, "analyzing their meaning, the character of their thought, the merits of their elocution." The French poets were read, and the students were required to discuss their ideas, style, and composition. "To learn poetry by heart was for me the same as learning or repeating fine pieces of music. That was not work." He loved to write his French compositions, and often those of his idler fellow-students.

Madame Guyot and her daughters had an institute for young ladies at Hauterive, a small village on the hillside overlooking the lake, with a magnificent view of the Alps. Here Lesquereux was made at home from the first year of his life in Neuchâtel. Here he and Guyot spent their Saturday nights and Sundays together. The shy boy came in time to take his part in the games and excursions of the girls; read and sang with them; found a mother, and fine company, and "some of that sweetheart disease which when enjoyed too young generally turns to disappointment or even distress."

His private lessons now began to pay him well, and he went to live in the same house with the brilliant and sociable Professor of Mathematics of the academy—a good painter, excellent musician and composer, and a most attractive talker, who had taken a fancy to the stupid boy who could not learn mathematics. They took their meals together, and he found his friend an invaluable aid in his study of philosophy. "Yes," writes Lesquereux, "I did not know how to think; but under his patient direction I tried and learned a little of the difficult process of thought." He learned stenography, and found it of great assistance to his memory; for his shorthand notes of the lectures were afterwards copied carefully, and discussed with his fellow-students. He spent much time in the public library, and came to be praised by his professor of philosophy as his best student, who had free access to him for advice.

Guyot had gone to a German university. Lesquereux's father could no longer support him at the academy. Neuchâtel and Geneva were "hot-beds" for the training and education of private teachers and nursery maids. Every year squads of these were sent in great slow-going omnibuses to Berlin, Vienna, and cities in Russia, and even as far as Persia, where they were installed in houses of people of consideration to teach children French and other accomplishments. One of Guyot's sisters, for instance, was governess to

the German Prince Imperial (*Unser Fritz*). Lesquereux was offered the place of private teacher in the family of the Governor of Tiflis. His friend the professor of mathematics advised him to decline so distant and unknown a post. He did however accept an offer from Eisenach, in Germany, to give lessons, three hours per day, to the children of Herr Eichel, "a tall, very polite, and very fine man, austere in aspect, but speaking French still more fluently than his wife, and still more interested to hear me speak about all that he thought could be of interest to me." He describes his pupils as "three fine young boys, apparently very intelligent, and already speaking some French, which they had learned from their nursery maid; the youngest ten years old." He was installed in a neighboring house, and boarded at the stage tavern.

His story of the life at Eisenach is delightful reading. He was treated with the utmost kindness and consideration, and had permission to use all the rest of his time in giving private lessons for his own advantage. He was immediately introduced as "the new French teacher" to the best people of the place: "State councillors, generals, majors, directors of mines, inspectors of forests, government employés with long German names, and all their wives, young ladies, &c. The Baron von Gersdorf spoke French very finely indeed, and was an enthusiastic lover of that language, and later became an intimate friend." To most of these people he gave instruction. Eisenach was the second town of the Grand Duchy of Weimar, and its aristocracy was on the same footing as that of the capital. "Those whose French language was elegant and of good pronunciation had chances to be in favor at court. For all these people of high society, therefore, a teacher of the French language was a valuable acquisition. There was one there already, but he had made a bad beginning—coming without introduction and becoming intimate with common people, himself of common type." But all was not *couleur de rose* with him. He gives an amusing account of the trouble he had with the high ladies of the place, who did not quite accept him as authority on mooted points of style and grammar. He wrote to his friend, the professor of philosophy, to send him books, and worked hard over the two great volumes of Cyran Duriner's "grammar of grammars," as he calls it, and made out of it a grammar of his own. At one time he had six classes of young ladies and two or three of gentlemen. A few months later he was called to give a course of lectures on the French language at the

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town college. His days were full of work. His evenings were free. He was made an honorary member of a club, took part in dancing parties, and frequently spent his evenings at the Von Gersdorfs. On Saturdays and Sundays the Baron was his companion in visits to the Wartzburg or on tours of exploration through the Thüringenwald.

His future father-in-law General von Wolffskeel von Reichenberg now steps upon the scene, as a short bent man, looking like an old farmer clothed in military garments, sad of figure, speaking little and rarely, but of most benevolent address, the commandant of Eisenach, with one company of soldiers in his barracks; an old soldier who had followed Napoleon's Saxon contingent to Russia, been wounded many times, now honored and beloved by all; with three boys in college destined for the army, and two girls, 16 and 10 years old, to whom Lesquereux gave two hours of French instruction in each week. He describes the lively and friendly mother, from whose frequent and garrulous visits to the school room he learned that a third and older daughter was with her uncle, Baron von Fritsch, one of the Grand Duke's ministers at Weimar and a colleague of Goethe.

This young lady had been remarkably well educated, was beloved by everybody, and was one of Goethe's favorites, to whom he never failed to make a birth-day present. Eighteen months after Lesquereux's appearance at Eisenach she returned to her home. They fell in love with each other, and were in due time married. This is no occasion for the recital of a romance, amusing as my learned audience would find it, told with the smouldering youthful enthusiasm of an octogenarian, and with the delicate wit and humor of a refined and cultivated heart; nor will I extract anything from his reflections on the woman life of Germany, nor any of the merry episodes of the wedding. Some day I will find occasion to print it all, and you can enjoy it then in the *ipsissimis verbis* of the dear old botanist himself.

Late in the spring of 1829 he returned to Fleurier—"a long, tedious, and tiring journey of ten days"—on foot, through the Black Forest and over the Jura mountains. He describes his approach, his shouts from afar, his mother's welcome, the grandmother's, the children's, the father's, the workmens' at the forge. "If there had been a fatted calf it would have been killed that day." A week later he learned of a vacant teachership at Le Lôle, a village of

6,000 inhabitants, 18 miles from Fleurier, on the high plateau. After less than one month the Board of Education acknowledged that the school had become quite orderly; that pupils and parents were satisfied, and that the post was his if he chose to accept it on a salary of 60 louis d'or (\$300) per annum. He hesitated. He could not marry on that. La Chaud de Fonds, eight miles further, had a college, and there would soon be a vacancy with a salary of 72 louis d'or. He had scarcely three months to prepare for a severe examination which would last a week. He was one of twenty-one candidates. At the close three only remained. Lesquereux won the prize, "but with a close shave." His early history, his life in Eisenach, his projected marriage with one of the Well-born, interested the people. He taught his pupils as if they were his own children. Once a week he took the school into the open and gave it object-lessons in natural history. "I knew a little botany and a little geology, too," he writes. "I had read much and liked to tell stories." "We always came back in the evening loaded with plenty of things—plants, flowers, stones, &c., good, of course, to throw away, but leaving after them a fine remembrance of nature and scientific enjoyment for the future. These summer days in the valleys of the high Jura have a charm which cannot be understood by people who live in a more favorable climate, and they are therefore enjoyed with keen delight by the inhabitants." In contrast, he adds a terrible story of his sufferings in one of the winter storms between Chaux de Fonds and Lôle.

He describes his life at Chaux de Fonds. The first of July he took the diligence for Eisenach. "Such meetings and such joys repay the anguish of years of absence. Why cannot we let the water of happiness ooze drop by drop to refresh our heavy hearts, restore courage, and cheer us by new hope when we have to traverse the barren, desolate places of our life? Why? We may. The means are with us: Remembrance." This is the widowed old man's cry at the end of one of his letters.

He was married on the 30th of July, and the couple returned to Chaux de Fonds in one of the omnibuses already described (being shamefully plundered en route at the Darmstadt custom-house), and set up housekeeping in one ill-furnished room. "We had a place for fire, but no regular kitchen. I cooked our breakfast and supper; we got dinner from a restaurant; and so we began." The young wife found her lonely life very hard, and was often in tears before

her husband returned from his college duties. They soon moved into a separate house, and the two following years passed without trouble. The upright piano arrived, and the first baby in the following May. The husband taught six hours a day in the college, taught private pupils besides, and spent many of his nights without sleep, nursing both the child and the mother. His income never surpassed \$500 a year. In Switzerland rent is high and hospitality costly. No call of a friend is received without offering food and drink to the visitor. The second year one of his wife's sisters came to live with them and complete her French education, which made their circumstances still more straitened. The young mother began to give music lessons; but by and by they had to part with all the objects of value which she had brought with her from Germany to meet the balance against them as each semi-yearly settlement day came round. The second child came with the third year, and was, according to the custom of the country, subjected to the *mail-lot*—that is, tightly swathed from head to foot. "Nobody could admit that a child could thrive, or even live a few days, without that implement of torture." A hard fight with the grandmother, supported by all the women, resulted however in setting the baby free, according to the new maxims of Dr. Uffland's book on the first physical education of children. "The excellent health and robust growth of my children," writes the old man, "have reformed in a great measure the old methods, which, like the patois and other things of the good old times, were still predominant in the Jura mountains; for we were at Fleurier, at La Chaux de Fonds, and even at Lôle, a kind of authority. I was consulted as much as if I were the pastor of the village; and, if not consulted, at least our mode of living was considered and adopted by many."

Now came bad times. His mother had become deaf from a sickness commencing as a common cold. He himself took a similar cold and for a time lost his hearing, followed by ulcers in the inner canal of the ears, from which he suffered terribly, without abandoning his college duties; these were cured the following summer at the hot baths of Larey, in the Valais; but his hearing became more defective and began to interfere with his class teaching. He ceased to hear recitations. He prepared courses of study, according to which he lectured, demanding of his pupils written notes, which he corrected afterwards at leisure; but this proved unsatisfactory to both pupils and parents. After correspondence with Dr. Itard, the

author of a book on diseases of the ear, he went to Paris and submitted to treatment. "The celebrated doctor," he goes on to say, "treated me shamefully. Demanding payment before making an examination of my case; fearing, he said, that I might leave Paris without paying his price, which was very high, he performed a first operation by liquid injections so strong as to produce an inflammation of the brain, and then refused to come to see me, saying that that was not his specialty, and that I must have another doctor." He was nursed by a sister of his father living in Paris; and when sufficiently recovered, having become almost totally deaf, he returned home "to begin a new career and try some kind of work for the support of his family." He had long contemplated this necessity, but he "could see no way out of the deep slough of despond which seemed about to engulf him." He could really do nothing with the scientific materials procured by his education. His wife had her music pupils, and he some private pupils, but that was little to the purpose. By the advice of a friend, a *guillocheur* (or watch-turning engineer) he learned the art. There were many *guillocheurs* at La Chaux who could earn far more than was paid to the principal of a school. The engine, however, cost \$500. The wife's piano was sacrificed and its place in the room taken by a "turning engine." Instead of going out of the house eight hours a day to teach, he now worked with his wife at home, without responsibilities to pupils or parents, and was well satisfied with the change. But he had to work hard for very little profit, and for the first year was "a poor, slow, unskilled workman." Although he had plenty of friends, the watch-case makers could not accept poor work for the same price as good work. And when spring came he "felt, like the birds at that season, the need of wandering out after flowers for his herb-arium, and rambling in the woods, after being a prisoner in a room for months." Poverty drove them from their commodious home into a cheaper quarter of the village. In two small rooms and a little kitchen they lived comfortably enough when work was plenty; but the country custom of paying for work only twice a year was sometimes distressing; and although he worked from 6 a. m. to 10 p. m., and sometimes even later into the night, he could only earn one dollar a day; and many times he had no work at all to do. I refrain from giving some touching and charmingly told stories in this canto of the idyl of his life, and omit also many interesting details about his employment which spread a color of reality over the

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romance. His wife after a while found a seat by her husband's work-bench, and during the last years of the sojourn at La Chaux her work was quite as valuable and remunerative as his own. But bending over the machine was gradually undermining his health, and his nerves were affected by the tremor of the machine. "Good workmen do not care about that," he writes. They earned enough to spend much of their time drinking wine and playing cards at the cabarets; but he could only spend his little spare time in fatiguing rambles in the woods. At last his marasmus got the better of him; he could no longer work; he could not walk the length of his room. He left his wife desperately, went to Fleurier, was nursed by his mother like a new-born child, and after ten days could walk for an hour in the meadow behind his father's house. Then his father offered to receive his family, and install himself as one of his own apprentices.

He begins his next letter to me with the sentence: "Still a new career to begin, and that one degree lower than any of the former ones; for I was now not even a day laborer." He then describes the hours of work, the way watch-springs were made, examined, packed, and marketed, and the part he took in the manufacture.

In spite of all this he was a man of science. "When I had not to help my father," he says, "I worked most of the time in my kitchen, being there alone or receiving visitors. How many friends, even men of celebrity, have remained sitting near me while I was coloring my springs, conversing quite as pleasantly as if we had been in a parlor. Dark and dirty as was this kitchen, I passed in it most of my time for years; never alone, for I had always something to think of. The work was merely mechanical, and the brain was in full activity without hindrance. Books of mine were written in that kitchen."

The first year at Fleurier was difficult and sad. They felt like poor strangers and beggars; and his nobly born German wife resented his position as a manservant more than he himself, a free-born Swiss, the son of an artisan, could do. In the fall of 1837 he gladly agreed that she and her two boys should visit her parents in Eisenach. Lesquereux was alone for a whole year. Their third child was born in Eisenach. When she returned with her three children, her youngest brother, and the sister who had formerly been with them at Chaux de Fonds, all was much changed. His apprenticeship was ended; he was a journeyman; and by and by

his father put into his hands the care and the management of the business. Accounts, correspondence, purchase of materials, sale of products were now his affairs. He placed his family in a small house and gave them as many comforts as his own parents enjoyed. For himself his deafness, if nothing more, cut him off from association with the workmen of the village. He spent no money at the club—at the tavern. He was a man of science, a botanist.

Now he began to take up again his botanical studies, and especially that of the mosses. He bought a microscope for \$5, "a very poor instrument, of course," and spent his evenings and portions of the night with it and a student's lamp. In this occupation he was joined by an enthusiastic naturalist, the old physician who had patched up his little body after the fall from the mountain top. With patients distributed in villages and on farms over an area with a radius of a dozen miles, this old doctor, who never owned a horse, had but little time to ride his hobby, and no one to help him or direct his studies; nor had he any special branch of natural history to prefer. Now, he made Lesquereux his companion; invited him to dine with him and his daughter frequently, or to coffee after dinner. The daughter was lame, nearly paralyzed by rheumatism, and loved botany. Lesquereux on Sundays gathered mountain flowers for her admiration and study, some of them rare. Conversation with father and daughter was easy enough, for at La Chaux de Fonds he had learned to read the movement-meaning of the lips and "rarely missed a word of conversation when the language was slowly pronounced."

He had another friend, a rich botanical amateur, with fine and peculiar flowers in a large garden, for the care of which he kept a gardener; but he never allowed a flower to be cut. Once he gave his wife a birthday rose and confessed that he never ceased to regret the act. Lesquereux, who had no spare time to study in the garden, got little good from it; but no matter. Did he not live in Fleurier, the flowery village? All around it was a splendid vegetation, and the most beautiful and rarest flowers abounded. The highest peak of the Jura, the Chasseron, 6,600 feet above tide, was quite near, and overlooked the Val de Travers. On its summit the Alpine flora begins to appear. Gentians, Ranunculas, Anemones cover the top in such abundance that in the beginning of July it looks like a real carpet of flowers. The climb from the village was only two hours, and it was his favorite excursion—at first alone, then with his boys

when they were large enough, and then with European botanists on a visit to him. The view of Switzerland with its four large lakes, its towns and villages, meadows and vineyards, is superb. The whole chain of snowy Alps stretches across the background of the picture. In after years, when the *heimweh* attacked him at Columbus, it was ever that prospect from the top of Chasseron that "floated for days and nights before his eyes, preventing sleep, thought, hunger, and calling him with an irresistible longing. Then I should have died," he adds, "but I went to the mountains of North Carolina and was cured. When I came in view of the Black Mountains, which are so like the Jura, I wept like a child, and the distressing anxiety passed away."

At this time his "very modest study of mosses" procured him the acquaintance of Schimper and other celebrated botanists of France. There was a poor boy of Fleurier who settled in Mühlhausen and became the wealthy merchant, M. Vaucheron, whose grounds were turned into a superb flower garden. Every year he visited Fleurier to collect, under Lesquereux's guidance, Alpine flowers for this garden. He and Schimper were great friends. Schimper, like Agassiz, or rather like all true naturalists, was poor, and never thought of procuring money to pay for his publications. When hard pressed, Vaucheron many times saw him through his troubles. Lesquereux received from him a present of the first numbers of Schimper's *Bryologia*. The next summer he brought Schimper to Lesquereux's workshop kitchen; looked over and corrected the specific names of Lesquereux's small collection of mosses, and gave him plenty of specimens of species which he had not. Thus continuing his microscopic researches in the night, sometimes during entire nights, Lesquereux soon became an authority for mosses; not a celebrity in science, but well-known for the fine specimens of rare species which he had collected and which he was prepared to exchange with correspondents. All these became good friends: Mougiot, Desmazieu, Lenormand, Scherer, Godet, Muhlenbeck, and others, who in successive years visited Fleurier, each spending one or two weeks in exploring the Jura mountains, as often as possible under his guidance. He could entertain but one himself; the rest found lodging at Mühlhausen, in Vaucheron's great mansion, as guests of the rich man with a very large heart."

Now, he says, began for Lesquereux a renovated existence, or rather, now he was to go up a new step of the ladder. Now began

his more earnest and fruitful study of those peat-bogs which extend so widely over the floors of the higher valleys of the Jura. The basin-like surface of the plateau on which is the village of Les Ponts, and on the southern edge of which is Desor's farm-house of Combe Varin, has a continuous peat bog twelve miles long. The parallel high plateau basins of Lôle and Chaux de Fonds, La Brevine, and La Chaux have still more extensive stretches of peat, from 4 to 50 feet deep. There is also a peat-bog in the Val de Travers, opposite the great spring of Noiraigue, and smaller ones between Motiers and Couret. Others lie at Les Verriers and Wraconne, towards the French border. These peat-bogs had an irresistible fascination for the bryologist of Fleurier, because on them he found the most beautiful species of mosses, and in great profusion, and because they administered to his natural passion for solitude. Even in their most dangerous places, which no other man or animal dared to visit, he felt at home among the now partly forgotten things of his youth. From Fleurier a pleasant climb and stroll of two hours would take him through the bogs of Les Sagnettes and La Brevine, bordering the lac d'Etailliers. A climb from Noiraigue took him to the bogs of Les Ponts, Lôle, and La Chaux de Fonds. If he had a whole day at his disposal he would walk to Neuchâtel and search the great level stretches of peat between the lakes of Neuchâtel, Bienne, and Morat. "What a splendid laboratory!" he exclaims at the beginning of the letter that opens this new chapter of his life. "What an immense amount of materials to use for study!"

I am at a loss to know how to follow his narrative. He describes the character and habits of the peat, in order to show why its study became so engrossing. The beautiful mosses which he had gathered from its surface had nothing in common with the bog on which they grew. The *Sphagnum* which made the bog was not a true moss at all. He longed to discover what it really was, and what was its function in the economy of nature. While pursuing his investigations the work-people of the Jura had become alarmed at the scarcity of fuel. The charcoal-burners were destroying the forests for the benefit of the French iron-works. Peat served the purposes of the villagers who lived along the borders of the bogs, but it was not acceptable to the Swiss who lived around the great lakes. Some clamored for the destruction of the bogs as useless obstacles to agriculture. Some pronounced them nests of malarious fevers, especially

the typhoid, so prevalent at La Chaux de Fonds. Some asserted that the evening fogs which covered the stretches of peat caused the spring frosts which killed potatoes and the few other vegetables cultivated in the high valleys. So great did the excitement rise that the Prussian Government offered a prize of twelve gold ducats, or a medal of that value, for the best memoir on the peat. Questions were formulated to which satisfactory answers must be given. The first question was: "What is peat?" The second: "How is it formed?" Others were: "Could peat-bogs grow, as some persons asserted?" "Is it true that once dug out from the bogs the matter would grow again in the open ditches?" "What influence have the bogs upon the atmosphere?" "Was peat a necessary fuel, or could wood be economically substituted for it?"

Agassiz had just then come to Neuchâtel and formed the Society of Natural History, of which Lesquereux had been made a member. Agassiz had married the sister of the celebrated German naturalist, Alexander Braun, and consequently took a lively interest in these botanical inquiries. He sent for Lesquereux, who found Agassiz hard at work dictating to Desor, and surrounded by his assistants, Vogt, Nicolet, Sornel, and his lithographers. The visit led to nothing but the acquaintance. Subsequent visits were few and short. Time was precious for both. He did not even speak to Agassiz of his intention to write a memoir. Indeed, he could never remember how the intention arose and took shape. He knew all the difficulties of the subject, all the time it would cost. He had no time to spare. His father's business could ill spare him for a single day. Many books must be read. He had never written anything as yet but "a poor catalogue of the mosses of Switzerland, which Schimper had helped him to prepare." "But the bogs called me and I went to them. I bought some old German books on peat and borrowed others. I procured some instruments, especially thermometers, and invented a sort of auger with which I could study the peat compound from the surface down to a depth of 12 or 14 feet, and the temperature at various depths." His father indulged him with extra leisure for the summer, so as to compete for the medal. His researches became more careful and precise. He established the mean temperature of the valleys by the depth at which his thermometers remained unchanged in the bog. He established thermometers at various stations and went from one to the other to record the data. He passed nights at a peat-digger's hut

in the middle of the Marais de la Brevine, observing the temperature of the air and bog outside, entering the hut now and then to warm himself. Going off at noon on Saturday morning, he brought back home, at noon on Monday, heaps of *Sphagna* to experiment upon as to their force and rate of absorption; either from below by their long filiform stems; or from above by their surface; weighing them; exposing them to different conditions of the air, in the cellar, in the garret, in sunshine, in bowls and plates on the ground, on the roof, and noting the effects of fogs; comparing their aspiration, their capacity for retaining moisture, &c.

The memoir was written in his kitchen workshop. It was sent to the council at Neuchâtel. A committee was appointed to examine it. The prize was awarded it. His joy was inexpressible. His parents were more happy than himself; "for, like most of the people of the village, who called me *the fool of the peat-bogs*, they could imagine nothing worth seeing or saying on that subject, nor that a son of theirs could know more about it than other people had done. 'Now,' said my father, 'do what you like henceforth; think good things for yourself and others; you shall always remain my partner, even if you should not work any more at watch-springs.' My dear wife only said, 'I knew you would do it.'"

Of the committee Agassiz was one; M. Coulon, a rich man and excellent naturalist, was another. One of the competitors for the medal was Nicolet, the geologist, who had publicly advocated the destruction of the peat-bogs, and published a pamphlet which adduced no facts. He positively denied Lesquereux's explanation of the peat formation, and the possibility of its spontaneous reproduction. The State council felt it a duty, therefore, to appoint a special committee of seven members to examine all the bogs. Agassiz, Nicolet, and Lesquereux were placed on this committee, which spent ten days in excursions and discussions.

Lesquereux writes: "This tour of exploration attracted me toward Agassiz far more powerfully than I can express; for he was a friend of Nicolet and somewhat inclined to admit his ideas; but all the time we were together he continued, like a true man of science, to examine every bit of evidence, questioning, wanting to know more of one thing or another, never offering contradiction, going on as a simple ignorant student, without any more pretense to superiority than a child. When the tour of exploration was finished the question was settled without any contesting voice. We

had a great dinner at one of the State councilmen's, and then Agassiz said to me: 'You have been found right in every one of your assertions; it is the vote we shall report to the State.'"

The Prussian government then requested Lesquereux to prepare a short manual for the use of all the Cantonal schools, to contain the outlines of the results of his first researches, and also clear, particular directions for the exploitation of the bogs of the high valleys and bogs of the low land; the two being of different nature, and therefore named by Lesquereux *emerged* and *immersed* peat-bogs; the latter, lying around the great lakes and at the mouths of the rivers, being formed by *cattail flags, pond weeds, water plantains, bog rushes, sedges, reeds, &c.*, all of them plants rooting at the bottom of permanent stagnant water basins, projecting their stems and leaves above water in summer, and decaying and sinking in successive layers to the bottom in the autumn.

For this school book he was paid \$500 and a commission to pursue his researches. This encouraged him to explore the peat-bogs of Europe, his father assenting to his absence for two months. The council also approved of his design and promised that the Prussian Government should pay his expenses and grant him facilities, letters patent to the local authorities of the Kingdom and letters recommending him to men of science in other countries.

In the beginning of July, 1845, he began at Mühlhausen, where his friend Vaucheron lived, and traversed the Vosges mountains to Strassburg, where Schimper was expecting him, to give him letters and show him the fossil plants in the museum of which he was director. He went from district to district in post-stages, but studied each district on foot. He found the bogs of the Vosges, like those of the Jura, composed of *Sphagna*, getting their moisture from the air and filling the highest vales to a level, but descending the valleys to the lowlands. He describes a striking peculiarity, however, viz. the growth of tufts of *Sphagnum* along the rapid streams which never made bogs. In after years he noticed the same peculiar growth of *Sphagna* in the Raccoon mountains of America, where there are no bogs, the summer heat drying the streams so that the mosses have no continuous growth, "but begin again each year from seeds or a kind of root (*prothallium*) which survives on the dry surface."

In the Vosges he encountered the celebrated botanist Mougeot, whom he had twice guided to the top of the Chasseron. Now,

Mougeot returned the favor by driving him to the Hühnecke, the highest summit of the Vosges, covered with fog and drenched in rain.

The impression produced on Lesquereux's mind by the greatest European collection of fossil plants at that time, in the Museum of Strassburg, was never effaced: He says: "I felt as if I had been transported into another world, and could scarcely leave the large room where the specimens were exposed. I said to Schimper, how happy a man should be with such an admirable vegetation to study! I did not dare to ask him for even a small piece of one specimen, although I should have prized the smallest as a treasure. Was it a remembrance of some former life, or a pre-vision of what was to come to me in the hereafter?"

From Strassburg to Mainz by steamboat, to Frankfort by rail, to Wurtzburg by steamboat on the Main, he reached Kissingen, from which the black basaltic knobs of the Röhn mountains rise, with the Kreutzbürg in their midst, holding aloft the hundred-foot-high iron cross of the Benedictine abbey. At Bischoffsheim he studied the lignite beds beneath the basalt, mined for fuel, fossil wood still with its normal aspect and easily cut with an axe. He describes the black underclay, similar to that of the floor of a peat-bog and holding fresh-water shells; but where the underclay failed the lignite, resting on basalt boulders, was crystalline and hard as coal. "Here then," he says, "I found a sort of transition from peat or wood to coal, and in my letters discussed the proof that coal was merely woody matter preserved against decomposition by certain circumstances, especially by the obstruction of oxygen, or of atmospheric influences. See my *Explorations dans le Nord*, 1846, page 15."

By the way of the coal mines of Ilmenau, near Weimar, of Permian age, he reached the lignite mines at Lützen, near Leipsig, called by the people *peat* diggings, and having the elements of very old peat-bogs; but the material, black and soft in some places, is in others hard and brittle, and should be called *Lignite*. At the bottom is a deposit of pine-tree trunks, black as coal, soft as clay, and mined with the shovel. The upper part of the mass has evidently been a peat-bog filling a deserted channel of the Saale. Here he saw that the flattening of tree trunks in coal beds has not necessarily been due to the enormous pressure of superincumbent deposits, but that the action of water so completely softens the

woody tissue of all plants that the small constant pressure of a few feet of boggy matter suffices to reduce the cylinder to a plate. In peat-bogs now growing the trees are mostly still hard and natural ; in the oldest bogs somewhat blackened ; but piles of old Roman bridges and roads across bogs in Switzerland have been found completely black and somewhat soft at the outside. The process of blackening, softening, and flattening is a very long one, and Lesquereux found opportunities to study all its stages. For instance, at Mächlenstadt, near Ilmenau, he saw the wood at the top of the deposit black but still hard ; at the bottom, layers of pine-cones with perfectly preserved forms with all their scales in place, but transformed into hard coal and considerably flattened. At Lützen he found pieces of hard brittle coal at the bottom of the trenches. In his *Letters of Explorations in the North*, published in the *Revue Suisse* in 1846, he gives a mature theory of the origin of coal, which has been adopted by all geologists who have studied coal measures, except a few who have been overinfluenced by another very different and exceptional set of local phenomena, like that at Commentruy, in France, and elsewhere.

Before leaving the Röhn mountains he examined the "Red bog" and the "Devil's bog," of which incredible stories were told, not merely by the superstitious peasantry, but by authors of some reputation ; but he found them precisely like those of the high Jura. I cannot take up your time with this amusing and instructive episode, a part of which has, however, a high scientific interest, as it relates to the remarkable dwarf pine trees (*Pinus pumillis*) which enrich the peat bog with their dripping pitch, and to which he assigned conjecturally the production of the Baltic amber and the pseudo-amber of the lignite of the Laramie group in America.

From Lützen he went to Berlin to get his letters patent from the Royal authorities, who received him graciously. He was cordially entertained by his wife's uncle, the Baron Manderode ; but saw neither Humboldt, who was in Paris, nor other scientific celebrities, who were off on their summer vacations. The chemist Mitscherlich, however, was very friendly, and gave him much information about the peat-bogs. The great botanist Link, director of the Botanical Garden, was too old and busy to converse much with a deaf man, but gave him a card of entrance and order for specimens.

A few days afterwards his uncle took him in his carriage to

Neustadt, at the base of the Hartz mountains. "That was really a charming pleasure party for me," he writes, "a day of real enjoyment." Thence he went on foot to Clausthall and had the curiosity to descend the copper mines. "I had little profit of that unscientific prank," is his only remark. But his journey through the Hartz to the top of the Brocken filled him with enthusiasm. Peat-bogs cover its base and ascend even on the most rapid slopes to quite near the summit. The sheet of granite boulders is covered with a treacherous sheet of moss. "There is no step where the foot can be placed with security."

Leaving the Hartz he passed through Braunschweig and studied the bogs of the Geest of Luneburg, the great plain which extends to the sea. He describes the wretchedness of the settlers, to whom the government had given small pieces of the bog to cultivate. At the village of Gifhorn they could give him nothing but black bread and milk and bitter beer, as black as ink. Here he first saw the peculiar peat of the shores of the North Sea and Baltic, a complex mixture of emerged and immersed bogs, like those which he afterwards examined at the mouths of the Elbe and Weser. He describes how these revealed the stratification of the coal measures—the superposition of sandstone on coal—by the breaking of wind dunes and the inundations of ice-choked rivers and invasions of the sea during great storms. These phenomena he studied in his course eastward, by way of Stettin, Schwinmund, and Greisewald. He compares these wide stretches of swamps to those of New Jersey, Virginia, and the Carolinas, describing them in detail. He especially desired to know whether some German observers were right or wrong who ascribed a notable part of the phenomena to the direct action of the sea, the heaping up of *fucus*, &c. After much trouble he obtained abundant evidence that there was no ground whatever for this opinion. He instances a subsequent experience in Holland where a village was reported to be using peat made of marine plants; but after a long detour he found that the people were wholly dependent on a scrubby wood and fallen sticks.

From Hamburg he went to Schevering, Wismar, Rostock, Strahlsund; went over to Scania, and through Istad and Malnive, reached Lund, where Neilson showed him the great Swedish Museum. He then returned to Denmark and examined its peat-bogs at and beneath the surface, holding stone, iron, copper, silver and gold, tools and ornaments, bones of animals, and entire skeletons of man.

Passing through Holland, his time and purse were both exhausted, and he reached his home at Fleurier long after the time appointed. Here he turned his diary into a series of letters for the *Swiss Review of Geneva*; and then the government of Neuchâtel bought a large tract of the peat land of Les Ponts, facing the Combe Varin, and made him superintendent of its exploitation. He was also appointed member of the commission for lowering the level of Lac Neuchâtel and draining the great swamp country between it and the Lac de Bienne, the Lac de Morat, &c. He worked hard, but had yet no definitive position beyond a promise of one.

While a committee of the Cantons interested in the draining of the lakes was sitting at Berne, a revolution broke out at Neuchâtel; the committee dispersed, Lesquereux went home to Fleurier. The Canton was severed from Prussia. The royal endowments to the Academy, the Council, &c., were lost, and all seemed to be at an end. What the new democratic order of things would produce none could predict. Agassiz had emigrated to America the year before; Guyot, Matile, and Desor soon followed him. The business at Fleurier, as everywhere else in the Canton, became too poor to support two families. His father agreed with Lesquereux that America was a more hopeful home for the children. His mother only objected. He does not even tell what port he sailed from. He ends his story with these touching words: "I came to that promised land a poor emigrant family, having an abominable voyage of sixty days in the entrepont (steerage) of a sailship, together with 300 companions of misery, the most terrible experiences of my life."

I am ignorant of the exact facts of his history for the next three years. He did not obtain the scientific work in Boston which he expected. He settled his family in Columbus, Ohio, where two of his sons established a business in the sale of watches, which still flourishes. The third son, I understand, died at Nashville, Tennessee. His daughter married a farmer near Columbus, and her children were his comfort in old age, after the death of his wife.

In 1851, Prof. H. D. Rogers employed Lesquereux to study the coal plants of Pennsylvania, and embodied his report on them with descriptions and figures of species in the second volume of the *Geology of Pennsylvania*, published in 1858. This memoir, entitled "*Fossil Plants of the Coal Strata of Pennsylvania*," occupies fifty quarto pages, and its quarto plates number twenty. Its one hundred and ten new species were established on specimens mostly

collected by himself, a few being based on specimens in the fine private collections of W. Clarkson, at Carbondale, in the anthracite, and of the Rev. Mr. Moore, at Greensburg, in the bituminous coal-fields. As many more known species were found in Pennsylvania. One hundred were European, and fifty others closely allied to European species.

At this time (1851) I became acquainted with Lesquereux, as he sat day after day on the anthracite coal-tips turning over each piece of waste slate in search of plant impressions. His patient zeal was a wonder to my impatient and restless nature. The broiling sunshine, the chilly wind, the soaking rain were alike disregarded by him. The evening brought him no repose, for his bag of specimens was exhibited, re-examined, discussed, and sometimes figured then. He was stone deaf, but he read the motions of our lips, and replied in a curious kind of English which he had never heard. Once he praised his wife to mine in these words, "A goot vife is the more better present as God can never geefe to some *body*," strongly accenting the last word, and his face shining with a light of love which never died.* A homelier, a more beautiful face I never saw. The homeliness was of the flesh; the beauty was the varying expressions of a perfectly lovable spirit. He was a little man with inexhaustible powers of life. His eyes were limpid; his smile heavenly; his gratitude for the smallest favors from men and his childlike confidence in the care of God unbounded. Everybody trusted and loved him.

* I have been present when Lesquereux talked with three persons alternately in French, German, and English by watching their lips. The interview would begin by each one saying what language he intended to use. He learned the English language after he became deaf; consequently his pronunciation of it was curiously artificial and original. I leave it to physiologists to explain how it happened that in the course of forty years the pronunciation of this deaf man gradually purified itself, so that in the end it did not differ greatly, except in certain words and only few of them, from that of the people about him. It is a remarkable fact well worth investigation. One of his friends, a member of this Academy, has just informed me that Lesquereux once told him that after Agassiz had resided many years in the United States, speaking English mostly, of course, his pronunciation of French was changed so that Lesquereux found some difficulty in reading his lips. The effect of English upon the muscular action of the mouth gradually modified the method of producing French sounds. The change was too subtle to be perceptible by those who only heard the words, but could be detected by the keen eye of a man stone deaf.

LEO LESQUEREUX.

Of course his services as a palæobotanist were from that time until his death in great demand. David Dale Owen employed him in the State surveys of Kentucky and Arkansas; Coxe, in Indiana; Worthen, in Illinois; Hayden, in the surveys of the Territories. Whitney submitted the California plants to his inspection. Innumerable private specimens were sent to him for identification. His cabinet of gifts and duplicates grew large and was in late years bought by the museum in Cambridge, Mass. All others were labeled and returned to their owners.

His "Coal Plants of Pennsylvania," already mentioned, was published in 1858, the same year in which he described species in the Boston Journal of Natural History. Those of Kentucky and Arkansas were published in Owen's Reports of 1860. The Tertiary plants of Mississippi appeared in Hilgard's Report of 1863. The coal plants of Illinois were described and figured in Worthen's Second Report of 1866 and Fourth Report of 1870. His Cretaceous Flora of the Dakota Group was published by Dr. Hayden in 1874. His Pliocene Flora of the Auriferous Gravel Deposits of the Sierra Nevada appeared in 1875. The coal plants of Indiana may be found in the Seventh and Thirteenth Reports of the Geological Survey of that State in 1876 and 1884. The coal plants of Alabama appeared in the State Geologist's Report of 1876. Descriptions of new species appeared in the Transactions and Proceedings of the American Philosophical Society in 1877 and 1878. The same year (1878) Hayden published Lesquereux's monograph "Tertiary Flora," and in 1883 another monograph, "The Cretaceous and Tertiary Flora."

All this represents but a portion of his work in America. Soon after settling in Columbus he became acquainted with a wealthy gentleman of that city, Mr. W. S. Sullivant, a zealous amateur bryologist, who eagerly seized on the fine opportunity to employ the most accomplished bryologist of Europe on the preparation of his memoirs of new species for publication. In the end this brought Lesquereux into friendship with the eminent bryologist of Philadelphia, Mr. Thomas P. James, who afterwards and until his death resided in Cambridge, Mass., and published with Lesquereux a Manual of the Mosses of America. Another of his close friends was Thomas C. Porter, professor of botany at Lafayette College, in Easton, Pa.

In 1861 he was elected a member of the American Philosophical Society, and in 1864, the year after its constitution by the United

States Government, a member of the National Academy of Science, whose meetings, however, he never attended on account of his deafness. He was a member of nearly a hundred other learned societies in Europe and America.

I have said that the works above referred to constituted but a part of his contribution to science. In fact, they may be considered as the American part of the preparation of the great palæobotanist for the crowning accomplishment of his life, a monograph on the Coal Flora of the United States. The year following the organization of the Second Geological Survey of Pennsylvania in 1874, Lesquereux was commissioned to prepare such a monograph. In 1877 he communicated to the proceedings of the American Philosophical Society in Philadelphia (Vol. XVI, pp. 397-416) a statement of the plan followed in his researches for its preparation, the available resources of information, the materials for it, when and where collected, the point in its progress then reached, and the more interesting results that would be exposed and illustrated by it.

The Coal Flora of Pennsylvania and the United States was published, as Report of Progress P, in 1880, volumes 1 and 2 bound in one, pp. 694, 8vo, with an atlas volume of 85 double 8vo plates of figures. Volume 3, continuing the paging up to 976, with tables and indexes, and additional double 8vo plates up to 111, appeared in 1884.

From this time until his death, in 1889, the old man scarcely ever left his home; was often ill; grew feebler, and found the examination of specimens more and more difficult; but until the spring of 1889 he kept up his correspondence with a few friends and did some work. His closest intimacy now was with Mr. R. D. Lacoe, of Pittston, in the northern anthracite coal-field of Pennsylvania, whose magnificent private collection of coal plants, fossil insects, &c. is so well known. The beautiful story of the fraternal—I should rather say, the parental and filial—relationship of these two amiable men, so made for one another, may some day be told; but I have no right to introduce it into this public address. They made each other protracted visits; letters and specimens passed to and fro between them incessantly. An appendix volume to the Coal Flora was in preparation. About a hundred new species, some of great rarity and beauty, were named and provisionally described; and Mr. Lacoe stands ready to edit the work if the State Survey of Pennsylvania can at any future time get authority to publish it.

MEMOIR
OF
ELIAS LOOMIS.
1811-1889.

BY
H. A. NEWTON.

READ BEFORE THE NATIONAL ACADEMY, APRIL 16, 1890.

BIOGRAPHICAL MEMOIR OF ELIAS LOOMIS.

ELIAS LOOMIS was born in the little hamlet of Willington, Conn., August 7th, 1811. His father, the Rev. Hubbell Loomis, was pastor in that country parish from 1804 to 1828. He was a man possessed of considerable scholarship, of positive convictions, and of a willingness to follow at all hazards wherever truth and duty, as he conceived them, might lead. He had studied at Union College, in the class of 1799, though apparently he did not finish the college course with his class. He is enrolled with that class in Union College, and he also received, in 1812, the honorary degree of Master of Arts from Yale College. At a later date he went to Illinois, and there was instrumental in founding the institution which afterwards became Shurtleff College.

Although the boy inherited from his father a mathematical taste, yet his love for the languages also was shown at a very early age. At an age at which many bright boys are still struggling with the reading of English, he is reported to have been reading with ease the New Testament in the original Greek. He prepared for college almost entirely under the instruction of his father. He was, for a single winter only, at the Academy at Monson, Mass. Owing in part to feeble health he was more disposed, in those early years, to keep to his books than to roam with other boys over the Willington hills. In his later life he frequently said that in his early days he never had a thought of asking what subjects he was most fond of, but studied what he was told to study.

At the age of fourteen he was examined and was admitted to Yale College, but owing to feeble health he waited another year before actually entering a class. In college he appears to have been about equally proficient in all of the studies, taking a good rank as a scholar, and maintaining it through his college course. President Porter remembers well the retiring demeanor of the young student, and his concise and often monosyllabic expressions, peculiarities which he retained through life. During his Junior and Senior

years he roomed with Alfred E. Perkins whose bequest was the first large endowment of the Yale College Library. He graduated in 1830.

A few weeks before graduation he left New Haven and entered a school, Mount Hope Institute, near Baltimore, to teach mathematics, and he remained there for a year and a term. One of his classmates, the late Mr. Cone of Hartford, said that Mr. Loomis had intended to spend his life in teaching, and that it surprised him when he heard that this purpose was abandoned, and that Mr. Loomis had gone, in the autumn of 1831, to the Andover Theological Seminary with the distinct expectation of becoming a preacher. This new purpose was, however, again changed when a year later he was appointed tutor in Yale College. A vacancy in the tutorship occurred in the May following (1833), and while not yet twenty-two years of age he returned to New Haven and entered upon the duties of the office. Here he remained for three years and one term. In the spring of 1836 he received the appointment to the chair of Mathematics and Natural Philosophy in Western Reserve College, at Hudson, Ohio. He was allowed to spend the first year in Europe. He was, therefore, during the larger part of the years 1836-'7 in Paris attending the lectures of Biot, Poisson, Arago, Dulong, Pouillet, and others. He did not visit Germany because of want of money. A long series of letters written by him at this time appeared in the Ohio Observer, and the contrast between England and France as he saw them and the same places as seen by the tourist to-day is decidedly interesting.

He purchased in London and Paris apparatus for his professorship, and the outfit for a small observatory, and in the autumn of 1837 began his labors at Hudson. Here he remained for seven years, maintaining with unflagging perseverance both his work in teaching and his scientific labors. In judging of this work at Hudson we must remember that he was not with perfect surroundings. He was without an assistant and without the counsel and encouragement of associates in his own branches of science. The financial troubles which culminated in this country in 1837 were peculiarly severe upon the young and struggling college. Money was almost unknown in business circles in Ohio, trade being almost entirely in barter. In this way principally was paid so much of the promised salary of \$600 per annum as was not in arrears. In one of his letters he congratulates himself that all of his bills that were more

than two years old had been paid. In another he says that there was not enough money in the college treasury to take him out of the State. When he left Hudson the college offered to pay at once the arrears of his salary by deeding to him some of its unimproved lands.

In 1844 he was offered, and he accepted, the office of Professor of Mathematics and Natural Philosophy in the University of New York. In this new position he undertook the preparation of a series of text books in the Mathematics, and for some years a large part of the time which he could spare from his regular college work was given to the preparation of these books.

When Professor Henry resigned his professorship at Princeton in order to accept the office of Secretary of the Smithsonian Institute, Professor Loomis was offered the vacant chair. He went to Princeton and remained there during one year, at the end of which he was induced to return again to his old place in the University of New York. Here he continued until 1860, when he was elected to the Professorship in Yale College, made vacant by the death of Professor Olmsted. For the last twenty-nine years of his life he labored for Yale College and for science, passing away on the 15th of August, 1889.

Let us look now in succession at the different lines of his activity during these fifty-six years—four in the tutorship at New Haven and in Europe; seven at Hudson, Ohio; sixteen in New York city and Princeton, and twenty-nine in New Haven.

For the first year on returning from Andover to New Haven he was tutor in Latin, although it seems that he might, had he chosen it, have been tutor of Mathematics. I believe that at the beginning his mind was not yet definitely turned towards the exact sciences. In his childhood he had taken specially to Greek. In college he was equally proficient in all of his studies. He is represented to have led his class at Andover in Hebrew, and now on entering the tutorship he chose to teach the Latin language and literature. During the second year he taught Mathematics, and the third year Natural Philosophy. His later success in scientific work was, I believe, in no small measure due to his earlier broad and thorough study of language.

I have made some inquiry in order to learn what it was that turned his intention and tastes towards science. One of his col-

leagues in the tutorship, the Rev. Dr. Davenport, says that he recollects very distinctly the first indication to his own mind that Tutor Loomis was turning his thoughts in this direction. The great meteoric shower of 1833 came early in the period of his tutorship, and the views of Professor Twining and Professor Olmsted about the astronomical character and origin of these interesting and mysterious bodies were a common topic of conversation among scientific men in the College, especially wherever Professor Olmsted was present. The tutors were accustomed to meet as a club from time to time in the tutors' rooms in turn, and Dr. Davenport well recollects the occasion when Tutor Loomis brought in a globe and discussed before the club the new theories about these bodies. Up to this time Tutor Loomis had seemed to him to have given his thoughts and study to language rather than to science.

In January, 1834, there were constituted in the Connecticut Academy of Arts and Sciences twelve committees, representing the several departments of knowledge, and Tutor Loomis was put on the Committee on Mathematics and Natural Philosophy. These are the only signs of scientific taste or activity which I have detected earlier than the autumn of 1834, after he had been a year and a term in the tutorship. From this time on to the end of his life he gave his time and energies to several subjects that are enough distinct one from the other to make it convenient to disregard a strictly chronological account of his labors, and consider his work in each subject by itself.

A subject of which he early undertook the investigation was *Terrestrial Magnetism*. We often use the rhetorical phrase "True as the needle to the pole," but, looked at carefully, the magnetic needle is anything but constant in direction; like the weather vane on the steeple, it is ever in motion, swinging back and forth, in motions minute and slow it is true, but still always swinging. It has fitfully irregular motions; it has motions with a daily period; motions with an annual period; and motions whose oscillations require centuries for completion.

The *daily* motions of the magnetic needle were those which Tutor Loomis first studied. At the beginning of the second year of his tutorship he set up by the north window of his room a heavy wooden block, and on it the variation compass that belongs to Yale College. Here for over thirteen months he observed the position of the needle

at hourly intervals in the day time, his observations usually being for seventeen successive hours of each day.

The results of these observations, together with a special discussion of the extraordinary cases of disturbance, were published in the American Journal of Science in 1836. No similar observations of the kind made in this country had at that time been published. So far as I am aware, none made before 1834 have since been published, except ten days' observations made by Professor Bache in 1832. In fact, I know of only one or two like series of hourly observations made in Europe earlier than these by Tutor Loomis. He also at this time formed the purpose of collecting all the observations of magnetic declination that had been hitherto made in the United States, and of constructing from them a magnetic chart of the country. He appealed successfully to the Connecticut Academy of Arts and Sciences for its sympathy and aid. The work of collecting facts was so far advanced before leaving New Haven that when he had been a few months professor at Hudson he forwarded to the American Journal of Science a discussion of the observations thus far obtained, and with them a map of the United States, with the lines of equal deviation of the needle drawn upon it. Two years later he published additional observations and a revised edition of this map.

These were the first published magnetic charts of the United States, and though the materials for their construction were not numerous, and in many cases those obtainable were not entirely trustworthy, yet sixteen years later, when a map was made by the United States Coast Survey from later and more numerous data, Professor Bache declared that between his own new map and that of Professor Loomis, when proper allowance had been made for the secular changes, the "*agreement was remarkable.*"

The northern end of a perfectly balanced magnetic needle turns downward, and the angle it makes with the horizon is called the magnetic *dip*. This angle is an important one, and is observed with accuracy only by using an expensive instrument, and taking unusual pains in observing. Hence only a few observations of this element were found by Professor Loomis. From these, however, he ventured to put on his first magnetic map a few lines that exhibited the amount of the *dip*.

While he was in Europe he purchased a first-class dipping needle, for Western Reserve College, and at Hudson and the neighbor-

hood in term time, and at other places in vacation, he made observations with this needle. Some of these observations were made before his second magnetic chart was published, and upon this map were now given tolerably good positions of the lines of equal magnetic dip. But he continued his observations for several years, determining the dip at over seventy stations, spread over thirteen States, each determination being the mean of from 160 to over 4,000 readings. These observations were published in several successive papers in the transactions of the American Philosophical Society at Philadelphia.

Various papers on terrestrial magnetism, in continuation of his earlier investigations, appeared in 1842, in 1844, in 1847, and in 1859, but movements in Germany, England, and Russia had meanwhile been inaugurated which led to the establishment by governments of a score of well equipped magnetic observatories, and this subject passed largely out of private hands.

Closely connected with terrestrial magnetism, and to be considered with it, is the *Aurora Borealis*. In the week that covered the end of August and the beginning of September, 1859, there occurred an exceedingly brilliant display of the Northern Lights. Believing that an exhaustive discussion of a single aurora promised to do more for the promotion of science than an imperfect study of an indefinite number of them, Professor Loomis undertook at once to collect and to collate accounts of this display. A large number of such accounts were secured from North America, from Europe, from Asia, and from places in the Southern Hemisphere; especially all the reports from the Smithsonian observers and correspondents, were placed in his hands by the Secretary, Professor Henry.

These observations and the discussions of them were given to the public during the following two years, in a series of nine papers in the *American Journal of Science*.

Few, if any, displays on record were as remarkable as was this one for brilliancy or for geographical extent. Certainly about no aurora have there been collected so many facts. The display continued for a week. The luminous region entirely encircled the North Pole of the earth. It extended on this continent on the 2d of September as far south as Cuba, and to an unknown distance to the north. In altitude the bases of the columns of light were about fifty miles above the earth's surface, and the streamers shot up at times to a height of five hundred miles. Thus over a broad belt

on both continents this large region above the lower atmosphere was filled with masses of luminous material. A display similar to this, and possibly of equal brilliancy, was at the same time witnessed in the Southern Hemisphere.

The nine papers were mainly devoted to the statements of observers. Professor Loomis, however, went on to collect facts about other auroras, and to make inductions from the whole of the material thus brought together. He showed that there was good reason for believing that not only was this display represented by a corresponding one in the Southern Hemisphere, but that all remarkable displays in either hemisphere are accompanied by corresponding ones in the other.

He showed also that all the principal phenomena of electricity were developed during the auroral display of 1859; that light was developed in passing from one conductor to another, that heat in poor conductors, that the peculiar electric shock to the animal system, the excitement of magnetism in irons, the deflection of the magnetic needle, the decomposition of chemical solutions, each and all were produced during the auroral storm, and evidently by its agency. There were also in America effects upon the telegraph that were entirely consistent with the assumption previously made by Walker for England, that currents of electricity moved from northeast to southwest across the country. From the observations of the motion of auroral beams, he showed that they also moved from north-northeast to south-southwest, there being thus a general correspondence in motion between the electrical currents and the motion of the beams.

When there is a special magnetic disturbance at any place, there is usually a similar one at all other neighboring places. But these disturbances do not occur at the several places at the same instant of time. Professor Loomis showed that in the United States they take place in succession as we go from northeast to southwest, the velocity of the wave of disturbance being over one hundred miles per minute. The waves of magnetic irregularities were thus connected with the electrical current and with the drifting motions of the streamers in the auroral display.

As incident to this discussion, he collected all available observations of auroras, and he deduced from them the annual number of auroras visible at each place of observation. These numbers, when written upon a chart of the Northern Hemisphere, showed that

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auroras were by no means equally distributed over the earth's surface. It was found that the region in which they occurred most frequently was a belt or zone of moderate breadth and of oval form, enclosing the North Pole of the earth, and also the North Magnetic Pole. It was therefore much farther south in the Western Hemisphere than in the Eastern. Along the central line of this belt there are more than eighty auroras annually, but on going either north or south from the central line of that belt the number diminishes.

In 1870 Professor Loomis published a paper of importance relating to terrestrial magnetism, in which he showed its connection and that of the aurora with spots on the sun. That the spots on the sun had periods of maxima and minima development had long been known. Lamont had noticed a periodicity in the magnetic diurnal variations. Sabine and Wolf and Gauthier had noticed that the two periodicities were allied. The connection of the period of solar spots with conjunction and opposition of certain planets had been shown by De La Rue and Stewart. Professor Loomis undertook an exhaustive examination of the facts that tended to confirm or refute the propositions that had been advanced. He confirmed and added to the conclusions of Messrs. De La Rue and Stewart. He also brought together such facts as were relevant to the question, and he showed that the regular diurnal variation of the magnetic needle was entirely independent of the solar spots, but that those disturbances that were excessive in amount were almost exactly proportional to the spotted surface of the sun. He also showed that great disturbances of the earth's magnetism are accompanied by unusual disturbances on the sun's surface on the very day of the storm.

Various forms of periodicity in the aurora have frequently been suggested. Professor Loomis, from all available accounts of the aurora, was able to show that while in the center of the zone of greatest auroral frequency auroras might be visible nearly every night, and hence that periodicity could not easily be shown by means of numbers of auroras recorded in such places, yet that such periodicity was distinctly traceable at places where the average number seen was about twenty or twenty-five a year. The times of maxima and minima of the solar spots were seen to correspond in a remarkable manner with the maxima and minima in the frequency of auroral displays in these middle latitudes. Also, from the daily observations made by Messrs. Herrick and Bradley at New Haven

during seventeen years, he concluded that auroral displays in the middle latitudes of America are generally accompanied by an unusual disturbance of the sun's surface on the very day of the aurora. The magnetism of the earth, the Aurora Borealis, and the spots on the sun have thus all three a causal connection, and apparently that connection is closely related to the conjunctions and oppositions of certain planets.

Shortly after the publication of this memoir, Professor Lovering published his extensive catalogue of auroras. A further discussion of the periodicity of the auroras was undertaken by Professor Loomis and published in 1873. In this he made use of all the auroras recorded in Professor Lovering's catalogue. They confirmed his previous conclusions, only slight modifications being required by the new facts presented, and by their more systematic collation.

In these papers, as in most of his papers upon other subjects, Professor Loomis was ever intent upon answering the questions: What are the laws of nature? What do the phenomena teach us? To establish laws which had been already formulated by others, but which still needed confirmation, was to him equally important with the formulation and proof of laws entirely new.

Let us now turn to another important line of Professor Loomis's work—*Astronomy*. As I have said, he was early interested in the shooting stars. In October, 1834, he read a paper before the Connecticut Academy of Arts and Sciences upon this subject, probably in substance that which was shortly afterward published in the American Journal of Science. The published paper is principally a restatement of the observations made in Germany in 1823 by Brandes in concert with his pupils for determining the paths of the stars through the atmosphere, together with methods of computation. From the results of Brandes's observations, however, he deduces an argument for the cosmic character of the shooting stars. One month after reading this paper to the Connecticut Academy he engaged in similar concerted observations with Professor Twining, who was then residing near West Point, N. Y. These were only moderately successful, but they were the first observations of the kind undertaken in America.

During the senior year of his college course there arrived at New Haven the five-inch telescope, given to the college by Mr. Sheldon Clark, constructed by Dolland. This instrument was much

larger than any telescope then in the country. It was temporarily placed in the tower of one of the college buildings, where it was mounted on castors and wheeled to the windows for use. This temporary abode it occupied, however, for over thirty years. In spite of its miserable location it was, in the decade following its installment, a power in the development of the study of astronomy in Yale College. The lives and works of Barnard, and Loomis, and Mason, and Herrick, and Lyman, and Chauvenet, and Hubbard, and of other graduates of the college prove this. What rich returns for Mr. Sheldon Clark's twelve-hundred-dollar investment!

In 1835 the return of Halley's comet had been predicted, and its appearance was eagerly expected by astronomers and the public: Professor Olmsted and Tutor Loomis first in this country caught sight of the stranger, and throughout its course they noted its physical appearances. With such means as he had at command, Mr. Loomis observed the body's place, and computed from his observations the orbit.

The latitude and longitude of an observatory are constants to be early determined. These were measured by President Day for Yale College in 1811. In the summer of 1835 Tutor Loomis, with such instruments as the college possessed—a sextant and a small portable transit—made numerous observations of Polaris for latitude, and several moon culminations for longitude. From these he computed the latitude and longitude of the Observatory tower. The longitude from Greenwich, though obtained from a small number of observations, differs less than two seconds of time from our best determinations to-day.

While in Europe in 1836-'37 Professor Loomis, as I have said, bought for Western Reserve College the instruments for an observatory. These were a four-inch equatorial, a transit instrument, and an astronomical clock. On his return he erected, in 1837, a small observatory at Hudson, and in September, 1838, began to use the instruments. He had no assistant, and by day had a full allotment of college work. Two hundred and sixty moon culminations and sixteen occultations observed for longitude, sixty-nine culminations of Polaris for latitude, along with observations on five comets, sufficiently extended for a computation of their orbits; these attested his activity outside of his required duties. Some years later, when the corresponding European observations were made public, he prepared an elaborate discussion of these longitude observations,

and published it in *Gould's Astronomical Journal*. A sixth comet was observed by him at Hudson in 1850.

It may not seem a very large output of work in six years' time to have determined the location of the Observatory, and to have observed five comets. But we must recollect that the telegraph had not then been invented, that the exact determination of the longitude of a single point in the western country had a higher value then than it can have now, and that it could be obtained only by slow and tedious methods. These were, moreover, days of small things in astronomy in this country. At Yale College there was a telescope but not an observatory. At Williamstown an observatory had been constructed, but it was used for instruction, not for original work. At Washington Lieutenant Gilliss, and at Dorchester Mr. Bond, were commissioned by the Government in 1838 to observe moon culminations in correspondence with the observers in the Wilkes exploring expedition for determining their longitude. These two prospective sets of observations, both of them under Government auspices and pay, were the only signs of systematic astronomical activity in the United States outside of Hudson, when in 1838 Professor Loomis began his observing there. In his Inaugural Address he asks: "Where now is our American Observatory? Where throughout this rich and powerful nation do you find a single spot where astronomical observations are regularly and systematically made? There is no such spot." Even when he left Hudson in 1844 the situation was not largely changed. Mr. Bond had removed his instruments and work to Cambridge. The High School Observatory at Philadelphia had been erected and Messrs. Walker and Kendall were using its instruments. Professor Bartlett had built the observatory at West Point, and had begun to observe there. Lieutenant Gilliss, after years of excellent work in the little establishment on Capitol Hill, had just finished the present Naval Observatory building at Washington; Professor Mitchel had begun to build the Cincinnati Observatory, and the Georgetown Observatory building had been erected. Professor Loomis's work at Hudson should be measured by what others were doing at the time, rather than by the larger performance of to-day.

In the summer of 1844, the year in which Professor Loomis came to New York, a new method in astronomy had its first beginnings. The telegraph line had just been built between Baltimore and Washington, and Captain Wilkes at Baltimore compared his chro-

nometer by telegraph with one at Washington, and so determined the difference of longitude of the two places.

Professor Bache was now Superintendent of the Coast Survey, and he determined at once to use the new method for the purposes of the survey. To Mr. Sears C. Walker was committed the direction of the work; but scarcely less important were the services of Professor Loomis, who for three campaigns had charge of the end of the lines in Jersey City and New York. Their first partially successful efforts were made in 1846, but the practical difficulties were overcome and entire success was obtained by them in 1847 and 1848. In these years the differences of longitude of Washington, Philadelphia, New York, and Cambridge were thus determined with an accuracy far greater than any previous similar determination whatsoever.

The next summer, that of 1849, Professor Loomis assisted in a like work to connect Hudson, Ohio, with the eastern stations. His observations of moon culminations at Hudson were thus available equally with those made at Philadelphia, Washington, Dorchester, and Cambridge for determining the absolute longitudes of Atlantic stations from Greenwich. It was not until 1852 that European astronomers began to use these telegraphic methods in measuring longitudes.

In 1850 Professor Loomis published a volume on the *Recent Progress of Astronomy, especially in the United States*. A first and a second edition were soon exhausted, and in 1856 the volume was entirely rewritten and very much enlarged. Some of the topics in these volumes were the subjects of articles communicated from time to time to the public in this Journal, *Harper's Magazine*, and other periodicals. Another important contribution to astronomy appeared in 1865—that is, his *Introduction to Practical Astronomy*. Eminent astronomers in England and America have expressed in the highest terms their praise of this book. Though it is now thirty-five years since its first appearance, and many treatises on the same subject, some elaborate and some elementary, have since been published, yet for an introduction to practical work I believe that a student will find this volume better than any other for his uses at the beginning of his course.

The increase of our knowledge in astronomy was, from first to last, an object of special interest to Professor Loomis. Before he left New York the income from his text-books enabled him to make

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to Yale College the generous offer of going to New Haven and working in an observatory at his own charges, provided a suitable observatory should be constructed and equipped for him. Unfortunately, the college was not able, although it was greatly desirous of doing it, to avail itself of his generous offer. Near the same time he joined with public-spirited citizens of New York in an effort to establish an astronomical observatory in or near that city, and for that purpose an act of incorporation was obtained from the New York State Legislature. After going to New Haven he always took the warmest interest in the plans of Mr. Winchester for the establishment of an observatory in connection with Yale University. His counsel and assistance have been instrumental, more than the public could know, in producing and preserving whatever of value has been developed in that observatory.

The science of *Meteorology* has, however, been that in which Professor Loomis has made the most important contributions to human knowledge.

Shortly after his graduation, in 1830, and before he entered upon the tutorship, there appeared the first of a long series of papers by Mr. Redfield, of New York city, upon the theory of storms. In the last year of his tutorship there appeared also the first of a like remarkable series of papers on the same subject by Professor Espy, of Philadelphia. Two rival theories were advocated by these two men, and these theories became the subject of no little discussion in scientific meetings and in scientific journals for a long period of years. Professor Loomis had, from their very inception, taken a warm interest in these discussions and the subject of meteorology, and in particular its central problem, the theory of storms, held in his thought and work the first place from that time to the day of his death.

In his visit to Europe the year before he went to Hudson, he purchased a set of meteorological instruments, and for several years in Hudson he steadily performed the naturally irksome task of making twice each day a complete set of meteorological observations. A few weeks after he entered upon his professorship in Hudson a tornado passed five miles from that place, and he went out immediately to examine the track and learn what facts he could that should bear upon the theory of the tornado. The results were valuable, but he was not altogether satisfied with them. They led

him, however, to undertake the discussion of one of the large storms that covered the whole United States.

For this purpose he selected the storm which had occurred near the 20th of December, 1836. Sir John Herschel had recommended hourly observations to be taken by all meteorological observers on four term days in the year—that is, observations for thirty-six successive hours at each equinox and each solstice. This storm fell partly upon one of these term days. Professor Loomis set to work to collect all the meteorological observations made during the week of the storm that he could obtain from all parts of the United States, and from some stations in Canada. The discussion resulting therefrom was read in March, 1840, before the American Philosophical Society at Philadelphia.

Let us for a little while consider the amount of knowledge of the facts about storms in our possession in 1840, the date when this memoir was read and an abstract of it published in Philadelphia. Franklin had noted the motion of storms from southwest to northeast. He said: * “ Our northeast storms in North America begin first in point of time in the southwest parts—that is to say, the air in Georgia, the farthest of our colonies to the southwest, begins to move southwesterly before the air of Carolina, which is the next colony northeastward; the air of Carolina has the same motion before the air of Virginia, which lies still more northeastward; and so on northeasterly through Pennsylvania, New York, New England, etc., quite to Newfoundland.” Redfield has traced several storms along the West India Islands northwesterly until about in the latitude of 30° their course was turned quite abruptly and they swept off northeasterly along the Atlantic coast toward and even past Newfoundland. Espy found some storms moving easterly or south of east from the Mississippi to the Atlantic.

Brandes had announced as a law that the wind in storms blows inward toward a center; but his law was an induction from a small number of observations. Dove had contended for a whirling motion; Redfield advanced facts to show that the winds blew in circles anti-clockwise around a center that advanced in the direction of the prevalent winds, and with him agreed Reid, Piddington, and others. Espy, agreeing with Brandes, claimed that the observations in the various storms showed a centripetal motion of the winds, toward a

* Letter to Alexander Small, May 12, 1760.

center if the region covered by the storm was round, and toward a central line if the storm region was longer in one direction than in another. Espy's conclusions were intimately connected with his theory that in the center of the storm there was an upward motion of the air, and that the condensation of vapor into rain furnished the energy needed for the continuation of the storm. The rival theories of Redfield and Espy were in sharp contest on several points, but the main contention was around this central question: Do the winds blow in circular whirls, or do they blow in toward a center? New York State was collecting observations from its Academies. The American Philosophical Society and the Franklin Institute, aided by an appropriation from the State of Pennsylvania, had united in an effort to learn the facts and the true theory of storms. Under such circumstances the thorough discussion of a single violent storm was likely to add materially to our knowledge. The treatment of this storm by Professor Loomis was probably more complete than that of any previous one, and the methods which he employed were better fitted to elicit the truth than any earlier methods. But the storm was a very large one, extending from the Gulf of Mexico to an unknown distance north, and having its center apparently to the north of all the observers. The results which he was able to secure did not sustain either of the two rival theories, but rather tended to prove some features in each of them. Professor Loomis was not himself satisfied with them, and he therefore waited for another storm that should be better fitted for examination.

In the month of February, 1842, a second tornado passed over northeastern Ohio, and Professor Loomis with one of his colleagues again started out for the examination of the track. The tornado passed over a piece of woods, and hence the positions of the prostrate trees showed clearly the motion of the wind in the passing tornado, and threw much light upon the character of this kind of storm. But the tornado was a single feature of a large storm that covered the whole country, and a second storm of great intensity was also experienced in the same month.

The discussion of these two storms was now undertaken by him. The paper giving the results of that discussion was sent to Professor Bache, and read by him at the centennial meeting of the American Philosophical Society, in May, 1843, and created, as Professor Bache wrote, a great sensation. It was at the time important for the light which it threw upon the rival contending theories

of Espy and of Redfield, but it was more important by far by reason of the new method of investigation which for the first time was then employed.

In the paper upon the storm of 1836 Professor Loomis had made some advance upon previous methods of representing the facts about storms. But even the method he then used was entirely unfitted to give answers to the questions which meteorologists were asking. Some of those questions were stated in the circulars issued by the joint committee of the American Philosophical Society and the Franklin Institute: What are the phases of the great storms of rain and snow that traverse our continent; what their shape and size; in what direction, and with what velocity do their centers move along the surface of the earth; are they round, or oblong, or irregular in shape; do they move in different directions in different seasons of the year?

The graphic representation by Professor Loomis on the map of the United States of the storm of 1836 had been a series of lines drawn joining the places where at a given hour the barometer was at its lowest point. That line would so far as the barometer was concerned mark for that hour the central line of the storm. The progress of the line from hour to hour on the map showed, though quite imperfectly, how the storm had traveled. Some arrows added showed to the eye also certain facts about the movements of the air.

Professor Espy adopted and thereafter adhered to a modification of this method of representing storm phenomena, and I think meteorologists will agree with me in my opinion that Professor Espy's four Reports from 1842 to 1854, though they contained an immense accumulation of facts, were because of this radical defect of presentation almost useless to meteorological science.

In the discussion of the storms of 1842, instead of the line of minimum depression of the barometer, Professor Loomis drew on the map a series of lines of equal barometric pressure, or rather of equal deviations from the normal average pressure for each place. A series of maps representing the storm at successive intervals of twelve hours were thus constructed, upon each of which was drawn a line through all places where the barometer stood at its normal or average height. A second line was drawn through all places where the barometer stood $\frac{1}{10}$ of an inch below the normal; and other lines through points where the barometer was $\frac{2}{10}$ below, $\frac{3}{10}$ below, $\frac{4}{10}$ below, etc.; also lines were drawn through those points where the barometer

stood $\bar{r}_0$, $\bar{r}_0^A$, $\bar{r}_0^B$, etc., above its normal height. The deviations of the barometric pressure from the normal were thus made prominent, and all other phenomena of the storm were regarded as related to those barometric lines. A series of colors represented respectively the places where the sky was clear, where the sky was overcast, and where rain or snow was falling. A series of lines represented the places at which the temperature was at the normal, or was 10 or 20 or 30 degrees above the normal, or below the normal. Arrows of proper direction and length represented the direction and the intensity of the winds at the different stations. These successive maps for the three or four days of the storm furnished to the eye all of its phenomena in a simple and most effective manner.

You have, no doubt, most of you, already recognized in this description the charts, which to-day are so common, issued by the United States Signal Service and by weather service bureaus in other countries. The method seems so natural that it should occur to any person who has the subject of a storm under consideration; but the greatest inventions are oftentimes the simplest, and I am inclined to believe that the introduction of this single method of representing and discussing the phenomena of a storm was the greatest of the services which our colleague rendered to science. This method is at the foundation of what is sometimes called "the new meteorology," and the paper which contains its first presentation stands forth, I am convinced, as the most important paper in the history of that science. I regret that I cannot aid my memory by quoting the exact words, but I remember distinctly what seemed to me an almost despairing expression made many years ago by one who had high responsibility in the matter of meteorological work, as he looked out upon the confused mass of observations already made, and felt unable to say in what direction progress was to be expected. With this I contrast the buoyant expressions of another officer charged with like responsibility, as he showed me, one or two decades later (in 1869), charts constructed like those of Professor Loomis, and said: "I care not for the mass of observations made in the usual form. What I want is the power and the material for making such charts as these." These two expressions of Sir George Airy and of Le Verrier mark the progress and the direction of progress in meteorology developed by Professor Loomis's memoir.

What was his own judgment of the method at the time of its

publication, and of its value in meteorology, can be seen from his words at the close of the memoir, which I beg permission to quote.

"It appears to me that if the course of investigations adopted with respect to the two storms of February, 1842, was systematically pursued we should soon have some settled principles in meteorology. If we could be furnished with two meteorological charts of the United States daily for one year—charts showing the state of the barometer, thermometer, winds, sky, etc., for every part of the country—it would settle forever the laws of storms. No false theory could stand against such an array of testimony. Such a set of maps would be worth more than all which has been hitherto done in meteorology. Moreover, the subject would be well nigh exhausted. But one year's observation would be needed; the storms of one year are probably but a repetition of those of the preceding. Instead, then, of the guerilla warfare which has been maintained for centuries with indifferent success, although at the expense of great self-devotion on the part of individual chiefs, is it not time to embark in a general meteorological crusade? A well-arranged system of observations spread over the country would accomplish more in one year than observations at a few insulated posts, however accurate and complete, continued to the end of time. The United States are favorably situated for such an enterprise. Observations spread over a smaller territory would be inadequate, as they would not show the extent of any large storm. If we take a survey of the entire globe we shall search in vain for more than one equal area which could be occupied by the same number of trusty observers. In Europe there is opportunity for a like organization, but with this incumbrance, that it must needs embrace several nations of different languages and governments. The United States, then, afford decidedly the most hopeful field for such an enterprise. Shall we hesitate to embark in it; or shall we grope timidly along as in former years? There are but few questions of science which can be prosecuted in this country to the same advantage as in Europe. Here is one where the advantage is in our favor. Would it not be wise to devote our main strength to the reduction of this fortress? We need observers spread over the entire country at distances from each other of not more than fifty miles. This would require five or six hundred observers for the United States. About half this number of registers are now kept in one shape or another, and the number by suitable efforts might probably be doubled. Supervision is needed

to introduce uniformity throughout, and to render some of the registers more complete. Is not such an enterprise worthy of the American Philosophical Society? The general government has for more than twenty years done something, and has lately manifested a disposition to do more for this object. If private zeal could be more generally enlisted, the war might soon be ended, and men would cease to ridicule the idea of our being able to predict an approaching storm."

This plan of a systematic meteorological campaign was cordially seconded by Professors Bache and Peirce. At a somewhat later date the American Academy of Sciences of Boston appointed a committee, of which Professor Loomis was chairman, to urge upon the proper authorities the execution of the plan. The American Philosophical Society of Philadelphia united its voice with that of the Academy. About this time Professor Henry was made Secretary of the Smithsonian Institution. He determined to make American meteorology one of the leading subjects of investigation, to be aided by the Institution. At Professor Henry's request, Professor Loomis prepared a report upon the meteorology of the United States, in which he showed what advantages society might expect from the study of the phenomena of storms; what had been done in this country towards making the necessary observations, and towards deducing from them general laws; and, finally, what encouragement there was to a further prosecution of the same researches. He then presented in detail a practicable plan for securing the hoped-for advantages in their fullest extent.

This plan looked to a unifying of all the work done by existing observers, a systematic supervision, a supplementing of it by new observers at needed points, a securing of the coöperation of the British government and the Hudson's Bay Company in the regions to the north of us, and finally a thorough discussion of the observations collected. A siege of three years was contemplated. In the history of the several steps that finally led to the establishment of the United States Signal Service this report has an important place.

The scheme laid down by Professor Loomis was in part followed out by the Institution. But the fragmentary character of the observations, the want of systematic distribution of the places of the observers, and the imperfections of the barometers made the material collected difficult of discussion. Professor Loomis waited in hopes of some better system.

In 1854 Professor Loomis undertook a rediscussion of the storm of 1836, using the new methods introduced for treating the storms of 1842. A visit to Europe shortly after enabled him to collect a large number of observations upon a storm or series of storms that occurred in Europe about a week later than that American storm. He had long been anxious to connect, if possible, these two storms, as he said, "stepping across the Atlantic." The European and the American storms, however, not only proved to be distinct one from the other, but the discussion showed clearly that many of the laws of American storms were radically different from those of the European storms. The results of the whole discussion were published in 1859 by the Smithsonian Institution.

Upon going to New Haven, in 1860, he commenced the collection of all the meteorological observations that had been made in New Haven and the immediate vicinity, and succeeded in finding sets which, when brought together, made up a nearly continuous record through 86 years. The results of these observations formed the subject of a memoir published by the Connecticut Academy of Arts and Sciences in 1866.

It became part of his duties in college to deliver a course of lectures upon the subject of meteorology. In preparation for these he caused to be printed in very limited numbers the outlines of a treatise upon meteorology, to be used as the basis of his series of lectures. In 1868 he developed this outline into a treatise suited to use in college classes and in private study. This treatise, notwithstanding the rapid advances of the science during more than twenty years, is still indispensable to the student of meteorology.

The better system of observing, for which Professor Loomis had been long waiting, came when the United States Signal Service was established, in 1871. The daily maps of the weather published by the Bureau were constructed essentially after the plan which Professor Loomis had, thirty years before, invented for the treatment of the storms of 1842. As soon as these maps had been published for the two years 1872 and 1873, Professor Loomis commenced in earnest to deduce from them the lessons which they taught us respecting the nature and the phenomena of United States storms. To this investigation he gave nearly all his energies during the remaining fifteen years of his life.

For several years he employed and paid for the services of assistants whose time was given to the preparation of material for use in

his studies. The aggregate cost of this assistance was of itself a very large contribution to science. Beginning in April, 1874, he presented regularly at each of eighteen successive meetings of this Academy, in April and in October of each year, a paper entitled "Contributions to Meteorology." These were at first based upon the publications of the Signal Service alone, but as years went by like publications appeared in Europe that were useful for his work. These papers were published in July and January following the Academy meeting, and they regularly formed the first and leading article in eighteen successive volumes of the *American Journal of Science*. Gradually, one after another of his college duties were committed to others that he might give his whole strength to these investigations.

An attack of malaria now interrupted the regularity of the series. His advancing years and diminishing strength warned him that the end of his investigations could not be far distant. The number of hours in which he could work each day was slowly diminishing. Five more papers followed at somewhat less regular intervals.

In 1884 he began a revision of the whole series of papers. They had been presented without much regard to systematic order in the subjects investigated, and new material had accumulated from time to time, so that a thorough, systematic revision seemed absolutely necessary.

In 1885 he presented to this Academy the first chapter of the revision, in which he discussed the areas of low pressure—their form, their size, their motions, and the phenomena attending them. Two years later, in 1887, the second chapter of the revision was presented, in which he discussed the areas of high pressure, their form, magnitude, direction and velocity of movement, and their relation to areas of low pressure. Gradually his physical strength was failing, though his mind was as bright and clear as ever. To this work, the only work which he was now doing, he was able to give two or three hours a day. Anxiously he husbanded his strength, slowly and painfully preparing the diagrams and the tables for the third chapter upon rain areas, the phenomena of rainfall in its connection with areas of low pressure, and the varied phenomena of unusual rainfall. "I see," he said to a friend, "not the end of this subject, but where I must stop. I hope I shall have strength to finish this work, and then I shall be ready to die."

This third and finishing chapter was finally passed through the

printer's hands and some advance copies distributed to correspondents abroad in the summer months of 1889. His work upon the theory of storms he felt was finished. As he paid the bill of the printer, he said to him: "When I return at the close of the vacation I expect to put into your hands for printing a new edition of the *Loomis Genealogy*." Before the close of the vacation he died.

These three chapters of his revised edition of *Contributions to Meteorology* constitute the full and ripe fruitage of his work in his favorite science. They will for a long time to come be the basis of facts by which writers in theoretical meteorology must test their formulas. They cover all the important points taken up in the twenty-three earlier memoirs, with one important exception, the relation of mountain observations to those made on the plains below. The laws connecting these two are not yet clearly indicated; much remains to be learned about them, and they are of the utmost importance in theoretical meteorology. He felt most deeply the backward steps taken by the United States Signal Service when mountain observations and the publication of the *International Bulletin* were discontinued. "The National Academy of Sciences," he said, "ought at once to take up the subject and use all its influence to secure the restoration of these two services."

Professor Loomis at various times studied certain other questions in physics and astronomy that were more or less allied with the subjects to which he gave the principal part of his time, and he published the results of his studies. He made a series of experiments on currents of electricity generated by a plate of zinc buried in the earth. He examined the electrical phenomena in certain houses in New York; the curious phenomena of optical moving figures; the vibrations sent out from waterfalls as the water flows over certain dams; the orbits of the satellites of Uranus; the temperature of the planets; the variations of light of the stars γ Argus and Algol; and the comet of 1861.

The subject of family *Genealogy* has a peculiar fascination for many minds. It would be an interesting study by a collection of facts to determine practically what are the elements in a man's character which lead him to engage in this peculiar study. Certain it is that men of most diverse disposition are led into it. I should not have thought it likely that Professor Loomis would have taken

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up the subject very seriously. Others have expressed to me the same thought, and he himself says that he did not think it strange that others should be surprised at his devoting so much time to this subject, for he was surprised at it himself. He became interested in the subject early in life, and that interest remained unbroken to his last days. For near forty years before his first publication he collected from time to time materials for a list of the descendants of his ancestor, Joseph Loomis, who came from Braintree, England, in the year 1638, and settled in Windsor, Connecticut, in 1639. In each of his four visits to Europe he extended his inquiries to his ancestor's earlier history in England. The materials thus collected were put in type in 1870. He published a list containing 4,340 descendants of Joseph Loomis bearing the Loomis name. He regarded it as entirely provisional, printed to help himself in making further researches, and to excite interest in others of the name, who would thus be led to give additional information, or correction of errors.

Finding that to a limited extent only could he hope by correspondence to gain the information desired, he now undertook in his vacations to canvass the country by personal visits. He collected lists of names from every available source—from catalogues of every description, from city directories, county directories, county maps, and county tax-lists—and he compiled from these sources lists of all the Loomis names he could find. Arranging these names by counties, he undertook to visit each family personally. In this way he made a pretty thorough canvass of every part of New England and New York State, of nearly every part of New Jersey and Pennsylvania, of the northern part of Ohio, and of some of the western cities.

After five years of these researches he published the second edition of the *Loomis Genealogy*, in which were given 8,686 names of persons that bore the Loomis name, descendants of Joseph Loomis in the male branches.

Five years later, in 1880, Professor Loomis printed in two additional volumes a provisional list of 19,000 descendants of Joseph Loomis in the female branches. Large as was this list, he did not regard it as more than a first outline of a census of the descendants of the original emigrant, and he hoped in the near future to publish an additional volume. For this he has left in manuscript many corrections and large additions that will be of use to the future Loomis Genealogist.

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Am I tarrying too long upon the vacation work of Professor Loomis? If so, I plead as a partial excuse that among these direct descendants of Joseph Loomis there were enrolled more than 200 graduates of Yale College, and nearly 100 more of its graduates have married members of this numerous family.

Professor Loomis was doubtless more widely known as the author of mathematical text-books than as a worker in new fields in science. Shortly after coming to New York he prepared a text-book in algebra. The market was ready for a good book of this kind, and the work prepared for it was a good one. It was followed the next year by a geometry. This was an attempt, and if judged by its reception and sale it was a successful attempt, to combine in a school-book the rigid demonstrations of Euclid with the courses of thought in Legendre and in modern science. The task is one of peculiar difficulty, as the existence and activities of the English Society for the Improvement of Geometric Teaching now for near twenty years illustrate. Other books followed the Geometry from year to year, the whole forming a connected series from Arithmetic upward, so that the list of his works finally numbered near twenty volumes. His experience in teaching, his rare skill in language, his clear conception of what was important, and his unwearied painstaking, combined to produce text-books which met the wants of teachers. About 600,000 volumes have been sold, benefiting the schools and colleges and bringing to the author a liberal and well-merited pecuniary return.

We ought not on this academic occasion to omit to speak of the teacher. College graduates who have been under his instruction will probably retain a more positive impression of the personal traits and the character of Professor Loomis than of most of their other teachers. His crisp sentences, lucid thought, exactness of language, and steadiness of requirement more than made up for any apparent coldness and real reserve. These characteristics of his riper years were peculiar to him from the beginning of his life as a teacher. During his tutorship he was thought to be strict as a disciplinarian, and this may have unfavorably affected his influence with some members of the class of 1837, in Yale College, of which he was tutor. It was not so with all of them. One of the members of that class, Chief Justice Waite, as he came to Commencement a few

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years since, occupying at the time the highest office which a lawyer in the line of his profession can in this country secure, said: "If I have been successful in life, I owe that success to the influence of Tutor Loomis more than to any other cause whatever."

There was in Professor Loomis so much of reserve that to many persons he seemed cold and without interest in the lives of others. But this was mainly due to appearances only. The tear would at times come unbidden to his eye. His correspondence with his classmates in the years immediately following graduation shows warm interest in all that concerned them. From Hudson he wrote often to Mr. Herrick, and complained much of isolation, but more especially of isolation from scientific companions and books.

In 1840 he married Miss Julia E. Upson, of Talmadge, Ohio, a lady about whom those who knew her have spoken to me only in terms of praise, and for whose memory Professor Loomis cherished a tender reverence. She died in 1854, leaving two sons. From this time Professor Loomis lived in apartments, surrounded by his books and devoted to his studies. His sons after passing their school and college days went to their own fields of work. During many years of his New Haven life he was unable to receive visitors in the evening. He made very few new friends, and one after another of his old ones passed away. To his work he was able to give undivided his time and his strength. His mind did not seem to require the excitement of social intercourse for its full and healthful activity. Isolated though he was, there was in him no trace whatever of selfish or morbid feeling. In council his advice was always marked by his clear judgment of what was important, and at the same time what was practicable. Whatever he himself had the right to decide was promptly decided by a yes or a no, and few persons cared to question the finality of his decision. But when his colleagues, or others, had the right to decide he accepted their decision without questioning or subsequent murmur. Upon being told that his letters to Mr. Herrick had come to the College Library, and that he could, if he chose, examine them and see whether there were among them any which he would prefer not to leave in this *quasi* public place, he promptly replied: "No; I never wrote a letter which I should be ashamed to see published."

After coming to New York he had a generous income from his books, besides his salary as professor. The amount he saved from his income was carefully and prudently invested, and before his

death the savings with their accumulations were a large estate ; how large only he and his banker knew.

To establish an Astronomical Observatory had been through life a cherished object. He entered into and aided heartily the plans of Mr. Winchester, both before and after Mr. Winchester asked his Trustees to transfer his magnificent endowment to Yale University. Professor Loomis looked forward to a large institution in the future on the observatory site. To endow this public service, after making liberal provision for his two sons, he bequeathed his estate. The income from more than \$300,000 will eventually be available to continue the work of his life. With clear judgment of what was most important, he limited the use of that income to the payment of salaries of persons whose time should be exclusively devoted to the making of observations for the promotion of the science of astronomy, or to the reduction of astronomical observations, and to defraying the expenses of publication. He knew that if he provided observers, other benefactors would furnish buildings and instruments and the costs of supervision and maintenance.

One of his college classmates told me that Mr. Loomis left college with the definitely expressed purpose that the world should be better for his living in it. The central proposition in his Inaugural Address at Hudson, in 1838, was: "That it is essential to the best interests of society that there should be a certain class of men devoted exclusively to the cultivation of abstract science, without any regard to its practical applications ; and consequently that such men, instead of being a dead weight upon society, are to be ranked among the greatest benefactors of their race." He chose this cultivation of science for his principal work for man, and he kept steadily through his whole life to the chosen work. Verily the world is better for the life and work of ELIAS LOOMIS.

PUBLICATIONS OF ELIAS LOOMIS.

1. On shooting stars. *Am. Jour.* (1), vol. 28, pp. 95-104. April, 1835.
2. Halley's comet (Olmsted and Loomis). *New Haven Daily Herald*, September 1, September 4, September 28, and December 31, 1835.
3. Halley's comet (Olmsted and Loomis). *Am. Jour.* (1), vol. 29, pp. 155-156. October, 1835.
4. Observations on the comet of Halley, made at Yale College. *Am. Jour.* (1), vol. 30, pp. 209-221. July, 1836.
5. Observations on the variation of the magnetic needle, made at Yale College in 1834 and 1835. *Am. Jour.* (1), vol. 30, pp. 221-233. July, 1836. (*Sturgeon's Ann. Electr.*, vol. 2, pp. 270-282.)
6. Letters from Europe. (Thirty-six letters.) *Ohio Observer* (1837).
7. Meteoric shower of November 13. *Cleveland Observer*, November, 1837.
8. Hourly meteorological observations for the December solstice of 1837, made at Western Reserve College. *Cleveland Observer*, December 23, 1837.
9. Hourly meteorological observations for the vernal equinox of 1838, made at Western Reserve College. *Cleveland Observer*, March, 1838.
10. Observations on a hurricane which passed over Stow, in Ohio, October 20, 1837. *Am. Jour.* (1), vol. 33, pp. 368-376. January, 1838.
11. Splendid meteor (May 18, 1838). *Cleveland Observer*, May 22, 1838.
12. Hourly meteorological observations for the summer solstice of 1838, made at Western Reserve College. *Cleveland Observer*, June, 1838.
13. On the variation and dip of the magnetic needle in different parts of the United States. *Am. Jour.* (1), vol. 34, pp. 290-309. (With a map.) July, 1838.
14. On the latitude and longitude of Yale College Observatory. *Am. Jour.* (1), vol. 34, pp. 309-313. July, 1838.
15. Meteors of August 9. *Cleveland Observer*, August 11, 1838.
16. An inaugural address, delivered August 21, 1838. 8vo, pp. 38. New York, 1838.
17. Hourly meteorological observations for the autumnal equinox of 1838, made at Western Reserve College. *Cleveland Observer*, September, 1838.

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(Professor Loomis also observed on the term days in December, 1838, and in March and June, 1839.)

18. On the meteor of May 18, 1838, and on shooting stars in general. *Am. Jour.* (1), vol. 3, pp. 223-232. January, 1839.

19. Meteorological table and register. *Am. Jour.* (1), vol. 36, pp. 165-173. April, 1839.

20. Observations to determine the magnetic dip at various places in Ohio and Michigan. In a letter to Sears C. Walker, Esq. Read June, 1839. *Am. Phil. Soc. Trans.*, vol. 7, pp. 1-6. (*Am. Phil. Soc. Proc.*, vol. 1, p. 116; *Am. Jour.* (1), vol. 38, p. 397.)

21. Astronomical observations made at Hudson Observatory, latitude $41^{\circ} 14' 37''$ north and longitude $5^{\text{h}} 25^{\text{m}} 42^{\text{s}}$ west, with some account of the building and instruments. (Latitude of observatory; moon culminations; occultations.) *Am. Phil. Soc. Trans.*, vol. 7, pp. 48-51. Read October, 1839. (*Am. Phil. Soc. Proc.*, vol. 1, pp. 129-130.)

22. Additional observations of the magnetic dip in the United States. *Am. Phil. Soc. Trans.*, vol. 7, pp. 101-111. Read October, 1839. (*Am. Phil. Soc. Proc.*, vol. 1, pp. 144-145.)

23. On the storm which was experienced throughout the United States about the 20th of December, 1836. *Am. Phil. Soc. Trans.*, vol. 7, pp. 125-163. (With three plates.) Read March, 1840. (*Am. Phil. Soc. Proc.*, vol. 1, pp. 195-198; *Am. Jour.* (1), vol. 40, pp. 34-37.)

24. Meteorological sketches. (Eight papers.) *Ohio Observer*, 1840.

25. On the variation and dip of the magnetic needle in the United States. *Am. Jour.* (1), vol. 39, pp. 41-50. (With a map.) July, 1840.

26. Observations to determine the magnetic intensity at various places in the United States, with some additional observations of the magnetic dip. *Am. Phil. Soc. Trans.*, vol. 8, pp. 61-72. Read November, 1840. (*Am. Phil. Soc. Proc.*, vol. 2, pp. 308-310.)

27. On the magnetic dip in the United States. *Am. Jour.* (1), vol. 40, pp. 85-92. (*Sturgeon's Ann. Electr.*, vol. 7, pp. 156-162.) January, 1841.

28. Meteorological observations made at Hudson, Ohio, latitude $41^{\circ} 14' 40''$ north, longitude $5^{\text{h}} 25^{\text{m}} 47.5^{\text{s}}$ west, during the years 1838, 1839, and 1840. (Barometer; thermometer and hygrometer; winds; rain.) *Am. Jour.* (1), vol. 41, pp. 310-330. October, 1841.

29. Astronomical observations made at Hudson Observatory, latitude $41^{\circ} 14' 40''$ north and longitude $5^{\text{h}} 25^{\text{m}} 45^{\text{s}}$ west. (Latitude of observatory; moon culminations; occultations; comet 1840, II; orbit of comet.) *Am. Phil. Soc. Trans.*, vol. 8, pp. 141-154. Read April, 1841.

30. On the dip and variation of the magnetic needle in the United States. *Am. Jour.* (1), vol. 43, pp. 93-116. April, 1842.

31. Encke's comet. *Ohio Observer*, April, 1842.

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32. On a tornado which passed over Mayfield, Ohio, February 4, 1842, with some notices of other tornadoes. *Am. Jour.* (1), vol. 43, pp. 278-300. (With a map.) April, 1842.
33. Supplementary observations on the storm which was experienced throughout the United States about the 20th of December, 1836. *Am. Phil. Soc. Trans.*, vol. 8, pp. 305-306. Read May, 1842.
34. Observations on the magnetic dip in the United States. *Am. Phil. Soc. Trans.*, vol. 8, pp. 285-304. Read May, 1842. (*Am. Phil. Soc. Proc.*, vol. 2, pp. 176-178.)
35. Icebergs in the Atlantic. *Ohio Observer*, July, 1842.
36. The comet. (Five papers, with orbit.) *Ohio Observer*, March, 1843.
37. On two storms which were experienced throughout the United States in the month of February, 1842. *Am. Phil. Soc. Trans.*, vol. 9, pp. 161-184. (With 13 maps.) Read May, 1843. (*Am. Phil. Soc. Proc.*, vol. 3, pp. 50-56.)
38. On vibrating dams. *Am. Jour.* (1), vol. 45, pp. 363-377. (Cuyahoga Falls; East Windsor; Springfield; Northampton; Gardiner; Hartford.) October, 1843.
39. Meteorological journal kept at Western Reserve College. (Forty-seven papers.) March, 1840, to January, 1844. *Ohio Observer*.
40. Modern astronomy. *New Englander*, vol. 2, pp. 3-18. January, 1844.
41. Comparison of Gauss's theory of terrestrial magnetism with observation. *Am. Jour.* (1), vol. 47, pp. 278-281. October, 1844.
42. Astronomical observations made at Hudson Observatory, latitude $41^{\circ} 14' 42.6''$ north and longitude $5^{\text{h}} 25^{\text{m}} 39.5^{\text{s}}$ west. Third series. *Am. Phil. Soc. Trans.*, vol. 10, pp. 1-15. Read November, 1844. (*Astron. Nachr.*, vol. 22, pp. 203-210, No. 517, October, 1844. *Roy. Astr. Soc., Month Notices*, December, 1844.) (Latitude of observatory; moon culminations; occultations; longitude of observatory; Encke's comet; comet of 1843; Mauvais's comet; Faye's comet.)
43. Meteorological observations made at Hudson, Ohio, latitude $41^{\circ} 14' 42''$ north, longitude $5^{\text{h}} 25^{\text{m}} 40^{\text{s}}$ west, during the years 1841, 1842, 1843, and 1844, with a summary for seven years. (Barometer; thermometer and hygrometer; winds; clouds; rain.) *Am. Jour.* (1), vol. 49, pp. 266-283. October, 1845. (*Astr. Nachr.*, vol. 22, pp. 203-210.)
44. Physical constitution of the moon. *Sidereal Messenger*, vol. 1, pp. 20-22. September, 1846. (*Am. Jour.* (2), vol. 2, pp. 432-433.)
45. A treatise on algebra. 12mo, pp. 346. New York, 1846.
46. On Biela's comet. *Am. Jour.* (2), vol. 2, pp. 435-438. November, 1846.
47. The planet Neptune. *Am. Review*, vol. 6, pp. 145-155. August, 1847.

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48. On the determination of differences of longitude made in the United States by means of the electric telegraph, and on projected observations for investigating the laws of the great North American storms. (Letter to Lieutenant Colonel Sabine.) *Phil. Mag.*, 3d series, vol. 31, pp. 338-340. August, 1847.

49. Notice of some recent additions to our knowledge of the magnetism of the United States and its vicinity. *Am. Jour.* (2), vol. 4, pp. 192-198. September, 1847.

50. Elements of geometry and conic sections. 8vo, pp. 222. New York, 1847.

51. Notice of a water-spout. *Am. Jour.* (2), vol. 4, pp. 362-364. November, 1847.

52. Report on the meteorology of the United States, submitted to the Secretary of the Smithsonian Institution. *Sen. Doc.* No. 23, 30th Congress, first session, pp. 193-207. Ordered printed January, 1848.

53. Historical notice of the discovery of the planet Neptune. *Am. Jour.* (2), vol. 5, pp. 187-205. March, 1848.

54. Note respecting Halley's comet. *Am. Jour.* (2), vol. 5, pp. 370-372. May, 1848.

55. The relations of Neptune to Uranus. *Am. Jour.* (2), vol. 5, pp. 435-437. May, 1848.

56. Elements of plane and spherical trigonometry, with their applications to mensuration, surveying, and navigation. 8vo, pp. vi, 148. New York, 1848.

57. Tables of logarithms of numbers and of lines and tangents for every ten seconds of the quadrant, with other useful tables. 8vo, pp. xvi, 150. New York, 1848.

58. On the determination of the difference of longitude by means of the magnetic telegraph. *Roy. Soc. Proc.*, vol. 5, pp. 787-789. 1849.

59. Experiments on the electricity of a plate of zinc buried in the earth. *Am. Jour.* (2), vol. 9, pp. 1-11. January, 1850. (*Am. Assoc. Proc.*, pp. 196-200, 1849. *Bibl. Univ., Archives*, vol. 13, pp. 265-281.)

60. On the longitude of Hudson (Ohio) Observatory. *Astron. Jour.*, Nos. 8 and 9. May, 1850.

61. On the proper height of lightning rods. *Am. Jour.* (2), vol. 10, pp. 320-321. (Read August, 1850.) (*Am. Assoc. Proc.*, pp. 38-43. 1850.)

62. On the electrical phenomena of certain houses. *Am. Jour.* (2), vol. 10, pp. 321-323. (Read August, 1850.) (*Am. Assoc. Proc.*, pp. 12-15, 1850. *Edin. New Phil. Jour.*, vol. 50, pp. 225-227.)

63. On optical moving figures. *Am. Assoc. Proc.*, pp. 293-295. 1850.

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64. The recent progress of astronomy, especially in the United States. 8vo, pp. 257. New York, 1850.

65. Elements of analytical geometry and of the differential and integral calculus. 8vo, pp. 278. New York, 1851.

66. On Kirkwood's law of the rotation of the primary planets. *Am. Jour.* (2), vol. 11, pp. 217-223. March, 1851.

67. Observations on the first comet of 1850, made at Hudson, Ohio. *Gould's Astr. Jour.*, vol. 1, pp. 179-180. March, 1851.

68. On the apparent motion of figures of certain colors. *Am. Assoc. Proc.*, pp. 78-81. 1851.

69. On the distribution of rain for the month of September. *Am. Assoc. Proc.*, pp. 145-149, 1851. (*Annual Sci. Disc.*, pp. 389-391, 1852.)

70. The elements of algebra, designed for beginners. 12mo, pp. 260. New York, 1851.

71. On the satellites of Uranus. *Am. Jour.* (2), vol. 14, pp. 405-410. November, 1852.

72. Notice of the hail-storm which passed over New York city on the 1st of July, 1853. *Am. Assoc. Proc.*, pp. 59-79, 1853. (*Am. Jour.* (2), vol. 17, pp. 35-55. *Annals of Science*, vol. 1, pp. 209-215.)

73. Does the moon exert a sensible influence upon the clouds? *Am. Assoc. Proc.*, pp. 80-83, 1853.

74. On the measurement of heights by the barometer. *Am. Assoc. Proc.*, pp. 169-171. 1853.

75. Comparison of the British Association Catalogue of Stars with the Greenwich Twelve-year Catalogue. *Gould's Astron. Jour.*, vol. 3, pp. 177-182. May, 1854.

76. On the resistance experienced by bodies falling through the atmosphere. *Am. Jour.* (2), vol. 17, pp. 67-70. July, 1854.

77. On the satellites of Uranus. *Am. Assoc. Proc.*, pp. 52-55. 1854.

78. The zone of small planets between Mars and Jupiter. (Lecture.) *Smithsonian Report*, pp. 137-146, 1854. (*Harper's New Month. Mag.*, vol. 10, pp. 343-353. February, 1855.)

79. An introduction to practical astronomy, with a collection of astronomical tables. 8vo, pp. 497. New York, 1855.

80. On the temperature of the planets, and on some of the conclusions resulting from this temperature. *Am. Assoc. Proc.*, pp. 74-80. 1855.

81. On the storm which was experienced throughout the United States about the 20th of December, 1836. *Am. Assoc. Proc.*, pp. 176-183. 1855.

82. Astronomical observations in the United States. *Harper's New Month. Mag.*, vol. 13, pp. 25-52. June, 1856.

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83. A treatise on arithmetic, theoretical and practical. 12mo, pp. 352. New York, 1856.
84. The recent progress in astronomy, especially in the United States Third edition; mostly rewritten and much enlarged. 8vo, pp. 396. New York, 1856.
85. On the relative accuracy of the different methods of determining geographical longitude. *Brit. Assoc. Rep.*, 1857 (2), pp. 25, 26. August, 1857.
86. On certain electrical phenomena in the United States. *Brit. Assoc. Rep.*, 1857, pp. 32-35; August, 1857. (*Pogg. Annalen*, vol. 100, pp. 599-606. 1857.)
87. Elements of natural philosophy, designed for academies and high schools, with three hundred and sixty illustrations. 12mo, pp. 344. New York, 1858.
88. On the electrical phenomena observed in certain houses in New York. *Am. Assoc. Proc.*, pp. 33-38, 1858. (*Am. Jour.* (2), vol. 26, pp. 58-62.)
89. On the variation of the magnetic needle at Hudson, Ohio. *Am. Jour.* (2), vol. 28, pp. 167-169. March, 1859.
90. Observations of the magnetic dip in the United States. *Am. Phil. Soc. Trans.*, vol. 11, pp. 181-186. Read August, 1859.
91. On certain storms in Europe and America, December, 1836, *Smith. Cont.* (accepted for publication August, 1859), vol. 11, pp. 26, and 13 colored charts.
92. The great auroral exhibition of August 28 to September, 1859. *Am. Jour.* (2), vol. 28, pp. 385-408. November, 1859.
93. On the European storm of December 25, 1836. *Am. Assoc. Proc.*, pp. 281-283. 1859.
94. Notices of the meteor of November 15, 1859. *Am. Jour.* (2), vol. 29, pp. 137-138, 298-300, and 447. January, March, and May, 1860.
95. The great auroral exhibition of August 28 to September 4, 1859—2d article. *Am. Jour.* (2), vol. 29, pp. 92-97. January, 1860.
96. The great auroral exhibition of August 28 to September 4, 1859—3d article. *Am. Jour.* (2), vol. 29, pp. 249-266. February, 1860.
97. The great auroral exhibition of August 28 to September 4, 1859—4th article. *Am. Jour.* (2), vol. 29, pp. 386-399. May, 1860.
98. The great auroral exhibition of August 28 to September 4, 1859, and the geographical distribution of auroras and thunder storms—5th article. *Am. Jour.* (2), vol. 30, pp. 79-100. July, 1860.
99. The great auroral exhibition of August 28 to September 4, 1859—6th article. (Selected from the Smithsonian papers.) *Am. Jour.* (2), vol. 30, pp. 339-361. November, 1860.

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100. The great auroral exhibition of August 28 to September 4, 1859—7th article. *Am. Jour.* (2), vol. 32, pp. 71-84. May, 1861.

101. The great comet of 1861. *Am. Jour.* (2), vol. 32, pp. 252-256. September, 1861.

102. On the great auroral exhibition of August 28 to September 4, 1859, and auroras generally—8th article. *Am. Jour.* (2), vol. 32, pp. 318-335. September, 1861.

103. On electrical currents circulating near the earth's surface and their connection with the phenomena of the aurora polaris—9th article. *Am. Jour.* (2), vol. 34, pp. 34-45. July, 1862. (On the action of electrical currents and the motion of auroral beams.)

104. Remarks upon the article of Prof. J. D. Everett. (On reducing observations of temperature.) *Am. Jour.* (2), vol. 35, pp. 31-34. January, 1863.

105. The elements of arithmetic, designed for children. 16mo, pp. 166. New York, 1863.

106. On vibrating water-falls. *Am. Jour.* (2), vol. 36, pp. 352-365. November, 1863. (South Natick; Holyoke; Lawrence.)

107. A treatise on astronomy. 8vo, pp. 338. New York, 1865.

108. The aurora borealis or polar light, its phenomena and laws. *Smithson. Rep.*, pp. 208-248, 1865. (*Archives Sci. Phys. Nat.*, vol. 31, pp. 273-285, 1868.) Rewritten and published with illustrations in *Harper's New Month. Mag.*, vol. 39, pp. 1-21. June, 1869.

109. On the physical condition of the sun's surface and the motion of the solar spots. *Am. Assoc. Proc.*, pp. 1-5. 1866.

110. On the period of Algol. *Am. Assoc. Proc.*, pp. 5-7. 1866.

111. Notices of auroras extracted from the meteorological journal of Rev. Ezra Stiles, S. T. D., formerly president of Yale College, to which are added notices of a few other auroras recorded by other observers at New Haven, Conn. *Trans. Conn. Acad.*, vol. 1, pp. 155-172.

112. On the mean temperature and on the fluctuations of temperature at New Haven, Conn., latitude $41^{\circ} 18'$ north, longitude $72^{\circ} 55'$ west of Greenwich (E. Loomis and H. A. Newton). *Trans. Conn. Acad.*, vol. 1, pp. 194-246. (Three plates.)

113. A treatise on meteorology, with a collection of meteorological tables. 8vo, pp. 305. New York, 1868.

114. Shooting stars, detonating meteors, and aerolites. *Harper's New Month. Mag.*, vol. 37, pp. 34-50. June, 1868.

115. A treatise on algebra, revised edition. 8vo, pp. 384. New York, 1868.

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116. Influence of the moon upon the weather. *Am. Assoc. Proc.*, pp. 118-122. 1868.

117. On the period of η Argus. *Roy. Ast. Soc., Month. Not.*, vol. 29, pp. 298-299. April, 1869.

118. Remarkable meteor of May 20, 1869. *Am. Jour.* (2), vol. 48, pp. 145-146. July, 1869.

119. Meteorology and astronomy, for academies and high schools. 12mo. New York, 1869.

120. Elements of astronomy, designed for academies and high schools. 12mo, pp. 254. New York, 1869.

121. The descendants of Joseph Loomis, who came from Braintree, England, in the year 1638 and settled in Windsor, Conn., in 1639. 8vo, pp. 292. New Haven, 1870.

122. Recent auroral displays in the United States. *Am. Jour.* (2), vol. 50, pp. 146, 147. July, 1870.

123. Comparison of the mean daily range of the magnetic declination, with the number of auroras observed each year, and the extent of the black spots on the surface of the sun. *Am. Jour.* (2), vol. 50, pp. 153-171. September, 1870. (*Archives Sci. Phys. Nat.*, vol. 40, pp. 352-358.)

124. Recent auroral displays in the United States. *Am. Jour.* (3), vol. 1, p. 309. April, 1871.

125. Recent auroral displays in the United States. *Am. Jour.* (3), vol. 3, p. 389. May, 1872.

126. The elements of analytical geometry, revised edition. 8vo, pp. 261, New York, 1872.

127. Instances of remarkably low temperature observed at New Haven, Conn. *Am. Jour.* (3), vol. 5, pp. 238-239. April, 1873.

128. Comparison of the mean daily range of the magnetic declination and the number of auroras observed each year, with the extent of the black spots on the surface of the sun. *Am. Jour.* (3), vol. 5, pp. 243-260. April, 1873. (*Palermo, Mem. Spettr. Ital.*, vol. 2, pp. 123-124.)

129. Results derived from an examination of the United States weather maps for 1872 and 1873. (With two plates.) *Am. Jour.* (3), vol. 8, pp. 1-15. Read in N. A. S. April, 1874. (Influence of rainfall upon the course of storms; influence of the wind's velocity upon the progress of storms; relation between the velocity of the wind and the velocity of a storm's progress; to determine whether a storm is increasing or diminishing in intensity; form of the isobaric curves; classification of storms; where do the storms which seem to come from the far West originate?)

130. Elements of the differential and integral calculus, revised edition. 8vo, pp. 309. New York, 1874.

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131. Results derived from an examination of the United States weather maps for 1872 and 1873. *Am. Jour.* (3), vol. 9, pp. 1-14. (With plate.) Read in N. A. S. November, 1874. (Direction and velocity of the wind within areas of maximum pressure; consequences of the outward flow of air from an area of high barometer; monthly minima of temperature; long-continued periods of cold weather; storm of January 6-8, 1874; connection between the velocity of the wind and the distance between the isobars in the neighborhood of a storm center.)

132. Results derived from an examination of the United States weather maps for 1872, 1873, and 1874—3d paper. (With plate.) *Am. Jour.* (3), vol. 10, pp. 1-14. Read in N. A. S. April, 1875. (Directions of storm paths; diurnal inequality in the progress of storms; influence of rainfall upon the course of storms; influence of a neighboring area of high barometer upon the progress of a storm; form of the isobaric curves; great and sudden changes of temperature; storm of January 15, 1875, at Denver, Colo.)

133. The descendants of Joseph Loomis, who came from Braintree, England, in the year 1638 and settled in Windsor, Conn., in 1639, second edition, revised and enlarged. 8vo, pp. 611. New Haven, 1875.

134. Key to treatise on algebra. 12mo, pp. 219. New York, 1875.

135. Contributions to meteorology, being results derived from an examination of the United States weather maps and from other sources—4th paper. (With plate.) *Am. Jour.* (3), vol. 11, pp. 1-17. Read in N. A. S. November, 1875. (Movement of areas of high barometer; monthly minima of temperature; influence of winds on the temperature, moisture, and pressure of the atmosphere; diurnal inequality in the rainfall; comparison of storm paths in America and Europe; oscillations of the barometer in different latitudes; storms traced across the Atlantic Ocean; velocity of ocean storms; storms of January 29 to February 8, 1870, on the Atlantic Ocean; application of Ferrel's formula; stationary storms.)

136. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—5th paper. (With two plates.) *Am. Jour.* (3), vol. 12, pp. 1-16. Read in N. A. S. April, 1876. (Low temperature of December, 1872; form of areas of maximum and minimum pressure; relation of rainfall to variations of barometric pressure; stationary storms near the coast of Newfoundland; course and velocity of storms in tropical regions.)

137. Elements of geometry, conic sections, and plane trigonometry. Revised edition, with appendix. 8vo, pp. 443. New York, 1876.

138. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—sixth paper. (With three plates.) *Am. Jour.* (3), vol. 13, pp. 1-19. Read in N. A. S. October, 1876. (Period of unusual heat in June, 1873; rain areas, their form, movements, distribution, &c.; rainfall of two inches at stations south of latitude 36°; rainfall of two inches at stations north of latitude 36°.)

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139. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—7th paper. (With three plates.) *Am. Jour.* (3), vol. 14, pp. 1-21. Read in N. A. S. April, 1877. (Rain areas, their form, dimensions, movements, distribution, &c.; areas of low pressure without rain.)

140. Key to elements of algebra. New York, 1877.

141. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—8th paper. (With two plates.) *Am. Jour.* (3), vol. 15, pp. 1-21. Read in N. A. S. October, 1877. (The origin and development of storms; violent winds; barometric gradient.)

142. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—9th paper. (With three plates.) *Am. Jour.* (3), vol. 16, pp. 1-21. Read in N. A. S. April, 1878. (Low barometer at Portland, Oregon; low barometer at San Francisco; areas of high barometer; temperature of Iceland and Vienna compared.)

(The above nine papers were translated by M. H. Brocard into French, and were published as No. 50 (2) of *Moigno's Actualités Scientifique*, Paris, 1880, with the title *Mémoires de Météorologie Dynamique*.)

143. A collection of algebraic problems and examples for the use of colleges and high schools in examinations and class instruction. 8vo, pp. 258. New York, 1878.

144. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—10th paper. (With two plates.) *Am. Jour.* (3), vol. 17, pp. 1-25. Read in N. A. S. November, 1878. (Storms of the Atlantic Ocean; fluctuations of the barometer on Mt. Washington and Pike's Peak; high winds on Mt. Washington; high winds on Pike's Peak.)

145. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and other sources—11th paper. (With two plates.) *Am. Jour.* (3), vol. 18, pp. 1-16. Read in N. A. S. April, 1879. (The winds on Mt. Washington compared with the winds near the level of the sea; abnormal storm paths.)

146. Anthony D. Stanley, professor of mathematics. In *Yale College, a sketch of its history*, &c., vol. 1, pp. 254-256. 1879.

147. Connecticut Academy of Arts and Sciences. *Ibid.*, pp. 329-337.

148. Contributions to meteorology, being the results derived from an examination of the observations of the United States Signal Service and from other sources—12th paper. (With three plates.) *Am. Jour.* (3), vol. 19, pp. 89-109. Read in N. A. S. October, 1879. (Mean pressure of the atmosphere over the United States at different seasons of the year; comparison of barometric minima in Europe and America; barometric minima advancing with unusual velocity.)

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149. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—13th paper. (With two plates.) *Am. Jour.* (3), vol. 20, pp. 1-21. Read in N. A. S. April, 1880. (Great and sudden changes of temperature; barometric minima across the Rocky Mountains; mean monthly range of the barometer.)

150. The descendants (by the female branches) of Joseph Loomis, who came from Braintree, England, in the year 1638 and settled in Windsor, Conn., in 1639. 2 vols., 8vo, pp. 1132. New Haven, 1880.

151. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—14th paper. (With three plates.) *Am. Jour.* (3), vol. 21, pp. 1-20. Read in N. A. S. November, 1880. (Course and velocity of storm centers in tropical regions; American storms advancing in a southeasterly direction; American storms advancing northerly and easterly; course of hurricanes originating near the Bay of Bengal, China Sea, &c.; rainfall in tropical cyclones; storms in the middle latitudes advancing in a westerly direction; storms advancing westerly over Europe and the Atlantic Ocean.)

152. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—15th paper. (With one plate.) *Am. Jour.* (3), vol. 22, pp. 1-18. Read in N. A. S. April, 1881. (Reduction to sea level of barometric observations made at elevated stations; height of the Signal Service stations.)

153. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—16th paper. (With a map.) *Am. Jour.* (3), vol. 23, pp. 1-25. Read in N. A. S. November, 1881. (Mean annual rainfall for different countries of the globe; cases of excessive rainfall; cases of deficient rainfall.)

154. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—17th paper. (With three plates.) *Am. Jour.* (3), vol. 24, pp. 1-22. Read in N. A. S. April, 1882. (Relation of rain areas to areas of low pressure.)

155. Contributions to meteorology—18th paper. (With a map.) *Am. Jour.* (3), vol. 25, pp. 1-18. Read in N. A. S. November, 1882. (Mean annual rainfall for different countries of the globe; relation of rain areas to areas of low pressure.)

156. Contributions to meteorology—19th paper. (With three plates.) *Am. Jour.* (3), vol. 26, pp. 442-461. Read in N. A. S. April, 1883. (The barometric gradient in great storms.)

157. Contributions to meteorology—20th paper. (With two plates.) *Am. Jour.* (3), vol. 28, pp. 1-17 and 81-93. Read in N. A. S. April, 1884. (Reduction of barometric observations to sea level.)

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158. Letter addressed to the Chief of the Bureau of Statistics in regard to the principal sources of the rainfall of different sections of the United States. *Report on the internal commerce of the United States*. Submitted May, 1885. Appendix No. 6, p. 208. Washington, February, 1885.

159. Contributions to meteorology—21st paper. (With a plate.) *Am. Jour.* (3), vol. 30, pp. 1-16. Read in N. A. S. April, 1885. (Direction and velocity of movement of areas of low pressure.)

160. Contributions to meteorology. *Nat. Acad. Sci. Mem.*, vol. 3, part 2, pp. 1-66. (Areas of low pressure, their form, magnitude, direction, and velocity of movement; also published as *Contributions to meteorology, revised edition*. 4to, pp. 1-67, plates 1-66. New Haven, 1885.)

161. Contributions to meteorology—22d paper. (With a plate.) *Am. Jour.* (3), vol. 33, pp. 247-262. April, 1887. (Areas of high pressure, their magnitude, and direction of movement; relation of areas of high pressure to areas of low pressure.)

162. Contributions to meteorology. *Nat. Acad. Sci. Mem.*, vol. 4, part 2, pp. 1-77 (with 16 plates). (Areas of high pressure, their form, magnitude, direction, and velocity of movement; relation of areas of high pressure to areas of low pressure;) also published as *Contributions to meteorology, chapter II, revised edition*. 4to, pp. 67-142, plates 17-32. New Haven, 1887.

163. Contributions to meteorology—23d paper. *Am. Jour.* (3), vol. 37, pp. 243-256. Read in N. A. S. November, 1888. (Relation of rain areas to areas of high and low pressure.)

164. Contributions to meteorology. *Nat. Acad. Sci. Mem.*, vol. 5, part 1. (Mean annual rainfall for different countries of the globe; conditions favorable to rainfall; conditions unfavorable to rainfall; examination of individual cases of rainfall in the United States, in Europe, over the Atlantic Ocean; areas of low pressure without rain;) also published as *Contributions to meteorology, chapter III, revised edition*. 4to, pp. 143-232, plates 33-51. New Haven, 1889.

MEMOIR
OF
JONATHAN HOMER LANE.
1819-1880.

BY
CLEVELAND ABBE.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1892.

BIOGRAPHICAL MEMOIR OF JONATHAN HOMER LANE.

Our late fellow-member, Jonathan Homer Lane, who died in the city of Washington May 3, 1880, was the eldest son of Mark and grandson of Joshua Lane, of the town of Strahan, in northern New Hampshire. His paternal grandmother's name was Tilton; his mother was Henrietta (Tenny) Lane, youngest child of Lieutenant John, who was a soldier of the Revolution, and Patience (Young) Tenny, of Bradford, Massachusetts.

The subject of this memoir was born at Genesee, Livingston county, New York, August 9, 1819. From the record of the class of 1846, Yale College, we learn that he attended school till but eight years old, his father being a farmer by no means well to do, and then, in the intervals of work (excepting three winters), enjoying only the instruction of his father and mother at home, notwithstanding which we find him later on engaged for nearly a year in teaching district schools. Obtaining the means of subsistence by teaching, our associate was enabled not only to enter Phillips' academy at Exeter, New Hampshire, in 1839, to prepare himself for college, but afterwards to enter Yale at the beginning of the sophomore year.

Possessing the qualities which distinguished him in after life, Mr. Lane during his college career rendered such assistance to many of his fellow-students from time to time in their studies in mathematics and natural science that they were not loth in after years to acknowledge to him their gratitude and sense of obligation.

After having been employed for a year as teacher at the seminary at Castleboro', Vermont, he was in 1847 appointed to a position in the Coast Survey, but on July 1, 1848, and on recommendation of Joseph Henry, he was appointed as assistant examiner in the Patent Office. On May 1, 1851, he was promoted to be principal examiner. He was thus honorably associated with the re-organization and development of this important bureau.

After ten years' service in the Patent Office "Mr. Lane was induced to withdraw," a step that is more exactly stated by his friend and colleague, Wm. B. Taylor, as follows: "In less than six months

"after the occupancy of the Interior Department by Jacob Thompson, of Mississippi, Judge Mason resigned office rather than permit either removals or appointments to be made in his bureau on grounds of political service. Within the same month (on September 20, 1857) Mr. Lane was removed from his post of examiner in the Patent Office."

As an examiner Mr. Lane was laborious and thorough, cautious and critical, conscientious in the extreme. Of course he could not be popular with the exterior world of inventors, but it may safely be said that no patent approved and endorsed by him has ever been successfully contested.

Throughout his official career as examiner and subsequently he continued to enjoy the appreciation, confidence, and friendship of Professor Henry, who always took great pleasure in his intercourse and in drawing out from the reserved and rather silent student his views on abstruse or doubtful physical questions.

Both were original members of a scientific and social club organized at Washington (in 1857?), and which continued its pleasant and instructive meetings for some dozen years, until in 1871 it developed into the Philosophical Society of Washington.

Many memoirs and problems presented to the Smithsonian Institution in the course of its extended correspondence (whether for publication or for solution), especially if involving mathematical principles or discussions, were referred by Professor Henry to Mr. Lane for his consideration and report, in the confidence that his judgment would never be found wanting.

After leaving the Patent Office, in 1857, Mr. Lane opened an office in Washington for the prosecution of business as an expert and counsellor in patent cases, and continued in this pursuit for several years. During this time he still devoted his leisure to researches in physical science, and also to the development of several ingenious contrivances having a practical bearing, though mainly designed as adjuncts to scientific inquiry. Among these he himself has mentioned "A machine for instantly finding, one after another, the real roots of the higher equations by electric currents in a set of small induction coils, a pretty illustration of the law of induction of a current upon itself and a unique method of using logarithms." He also devised and brought to practical operation an electric governor or a "Machine for very exact, uniform motion, controlled by electro-magnetic break-circuit." Another contrivance of his, prob-

ably referable to about this epoch, was a "Visual telegraph, capable of transmitting to any visible distance and of expressing itself far more rapidly than the pen in common writing."

As he had given considerable attention to the mathematical theory of electric currents, he attempted to work out experimentally "the law of the induction of electric currents as regards distance;" also to determine "the relation of the force of induction of electric currents to the force of resistance in a specimen of copper wire conducting a current." He undertook the construction and arrangement of apparatus for the more exact determination of the quantitative relation between static electricity and voltaic electricity, but it is believed that these experiments were never carried through to a satisfactory conclusion.

Mr. W. B. Taylor states that "in 1859, at the request of Professor Henry and under his suggestion, our colleague was led to undertake a series of experiments on various points of scientific interest relating to the conditions and special action of the Atlantic telegraph cable. Only a brief notice of this investigation occurs in the report of Professor Henry to the Board of Regents of the Smithsonian Institution for 1859, and we know of no further notes or papers detailing the results arrived at."

But legal business in Washington ceased to be profitable and an opportunity offered in the new petroleum fields of Venango county, Pennsylvania, to further both science and fortune: thither he went in 1860 or 1861, intent on pursuing both these objects.

It is interesting to note that so early as before entering Yale College he had speculated on the means of attaining a lower temperature for scientific purposes than had as yet been effected. This project, frequently recurring, was taken up in a practical form about the year 1865, during his residence in Pennsylvania, where he endeavored to have constructed a machine for effecting the condensation and rarefaction of air in large quantities. One of the objects more particularly in view in this research was to obtain, by investigating the conditions of successive compressions and expansions of gases, a closer determination of the absolute zero of temperature than was already reached.

In the spring of 1866, having realized, as he thought, a handsome sum from the sale of oil lands, he returned to Washington to devote himself to science and unfolded his project and its methods to his old friend Professor Henry, who undertook to forward his designs

by having apparatus specially constructed for his use by funds of the Smithsonian bequest devoted to researches of such a character. A series of experiments was made with a turbine specially designed by Mr. Lane and operated, under General Meigs' permission and direction, by the water-power of the Washington aqueduct mains. This work was based upon correct principles and must be considered as in the main successful, but Mr. Lane was too careful in attending to every detail or effective condition to be a rapid worker, and his fastidious desire for extreme accuracy led him to postpone the publication of results until much of the valuable work which he actually accomplished has, as is feared, come to be but imperfectly preserved. The failure of his banking firm sadly diminished his available funds and eventually caused the work to stop.

"In addition to the various activities thus imperfectly noticed he occupied himself with an extension or modification of his visual telegraph or semaphore, the object of which was to obtain 'A visual method by which time can be compared between stations from fifty to a hundred miles apart to one-thousandth of a second and the comparison carried on over a line of such stations.'" Apparently this method is appropriate for ascertaining differences of longitude in geodetic surveys.

Another project on which he expended some thought was "a mechanism to be used on shipboard for holding the Drummond light, with its lens or parabolic reflector, so that its concentrated beam of parallel rays shall rest motionless upon any desired point, and so that it can be manipulated with as much facility as on land."

He also perfected at a later date an "improvement in the artificial mercury horizon, whereby ripples and undulations of the surface were effectually suppressed." This was invented for the use of the Office of Weights and Measures, in whose employ he then was, and a full description was published in the annual report of the Coast Survey for 1871, appendix 16. An early account of this apparatus was communicated to the American Association for the Advancement of Science at its meeting in Troy in 1870.

During the first few years after his return to Washington, viz., from 1866 to 1869, Mr. Lane's time was fully occupied with private business, and especially with the experiments on the mechanical production of low temperatures. During this period was composed his admirable memoir on the "Theoretical Temperature of the Sun," read before this Academy in April, 1869. On November 10, 1869,

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Mr. Lane accepted a position under the Superintendent of the Coast Survey as "Verifier of standards in the Office of Weights and Measures." This was a peculiarly congenial position, and he retained it until his death, in 1880, not only rendering valuable service in the matter of detail with which he was charged, but originating general improvements and displaying here, as in every trust he held, both originality of thought and the most discriminating judgment. The Superintendent of the U. S. Geodetic and Coast Survey (our late fellow-member Julius E. Hilgard) on the occasion of announcing his death before the Philosophical Society of Washington thus expressed his estimate of the mental grasp and perspicacity of Mr. Lane: "The quality of Mr. Lane's mind was truly remarkable, being chiefly characterized by an extraordinary precision of thought and logic, and it was unfortunate that he lacked fluency of speech. Of the quietest, most retiring disposition, he was personally known to but few; diffident in manner and not given to many words, yet when he did speak his rare logic was such as to carry conviction. In his writings his clearness of mind became manifest; so lucid was it and so fully were those qualities and soundness of precision appreciated that his collaborators never thought an induction safe until it had passed through the alembic of his criticism.

"His exceedingly generous disposition led him to devote a considerable portion of his income to the assistance of two sisters and a brother; and in order that he might extend to these all the aid in his power he never married." *

From the nature of his occupations, in the Patent Office and in the Office of Weights and Measures, Mr. Lane was obliged to give much of his time to matters of office routine and detail, but he still found time to make many valuable experiments with expensive apparatus, the cost of which he himself defrayed, and also to publish a number of valuable and interesting memoirs in the domain of physics and of precise measurement. Valuable unpublished memoirs were also left by him. The high order of his mind is well shown in two important papers read before this Academy "On the Physical Constitution of the Sun" and "On the Determination of the Volume of a Sphere." His determination of the "Coefficient of Expansion of the British Standard Yard" was published after his death by the Coast and Geodetic Survey.

* Bulletin of the Phil. Soc. of Washington, May 8, 1880, vol. iii, p. 123.

An unpublished paper "On the Means of Measuring the Tidal Change in the Direction of the Plumb-line and the Tidal Deflection of the Earth's Crust" was finished in 1874. This shows that he was an early student of problems that are now being actively discussed. It seems likely that even at this late date this and other memoirs of Mr. Lane may be worthy of publication.

Mr. Lane's memoir of April, 1869, on the "Physical Constitution of the Sun" was published in June, 1870, under the title "On the Theoretical Temperature of the Sun," and its importance has been fully recognized by Peirce, Newcomb, Ball, William Thomson, Young, and others. In 1882 it was referred to by Newcomb and in 1887 by Thomson as demonstrating a paradox—namely, that the more heat a body loses the hotter it will become; but it must be confessed that no such paradoxical statement appears in the text of Mr. Lane's published memoir, and it should, perhaps, rather be referred to Prof. Benjamin Peirce, who in 1879 communicated to the American Academy certain propositions in cosmical physics (*Proc. Am. Acad.*, XV, p. 201), among which the following occurs: "Gaseous bodies in the process of radiating light and heat condense and become hotter throughout their mass." (See also Peirce, "Ideality in the Physical Sciences," Boston, 1881, pp. 160-198.)

Mr. Lane's memoir accepts Helmholtz's theory of the meteoric origin of solar heat and the subsequent maintenance of the solar temperature and radiation by a process of slow gravitational condensation upon itself. He applies this theory to a gaseous sun as distinguished from the solid or liquid sun conceived of by former investigators and deduces the distribution of temperature, density, and pressure that must obtain throughout such a sphere of gravitating gas if it be in convective equilibrium. Although it may be possible to deduce from Mr. Lane's formulæ a demonstration of the extent to which the condensation due to cooling increases the mutual gravitation of the gaseous particles and produces heat that compensates for the heat lost by radiation, yet Mr. Lane does not do this, as he had in mind a different object. He deals only with a static condition and attempts to determine approximately the present temperature and density of the solar gas. It would seem as if he had effected a solution of the problems suggested by Sir William Thomson in 1862 in his essay "On the Age of the Sun's Heat," wherein he says: "Mutual gravitation between the different parts of the sun's contracting mass must do an amount of work which

cannot be calculated with certainty, only because the law of the sun's interior density is not known." And again: "There must be an approximate convective equilibrium of heat throughout the whole, if the whole is fluid." But we are not restricted to the hypothesis of convective equilibrium, and Mr. Lane states definitely the difficulties in the way of accepting simultaneously both this modified Helmholtz theory and the mechanical theory of heat of Clausius. He shows that either the solar gases must be very different from those that occur on the earth or else that our idea of the nature of heat must be modified, and concludes with the statement that "of course this difficulty does not present itself when we suppose that heat is not motion."

During the last few years of Mr. Lane's life he suffered much from illness. His death, on May 3, 1880, called forth many eloquent and earnest expressions of regard from those who knew him. The following tribute from his pastor, Dr. Byron H. Sunderland, is well worthy of preservation:

"Of the propriety, integrity, and simplicity of his life, of his exceeding conscientiousness and carefulness and his modest shrinking from all self-assertion or ostentation, we all well know. He was not what we should style a demonstrative man. He lived quietly within himself, and his life was engrossed in scientific pursuits.

"The nature and construction of his mind was purely mathematical. This was evident in the exactitude of his language, even in the most casual conversations and the most trivial subjects. His speech was slow, but exceedingly discriminating and perspicuous. This was the character of his mind and all his mental habits. I doubt if there ever was among scholars a more striking example of carefulness of expression in the use of terms to indicate the exact shade of thought he designed to convey.

"He was very slow in all his processes and would never deviate in his reasoning or his language from the precise mathematical cast in which all his conclusions were ultimately conceived.

"Such an intellectual nature must of necessity be connected with a moral disposition which would perceive at once the relation and fitness of moral truth to the highest needs of man; and this he did perceive and accept with the simplicity of a child. His was a mind never to be borne away by the mere power of enthusiasm or any undue excitement of emotion. He viewed everything in the calm,

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clear light of the rational understanding, and only in this way could moral and religious truth impress him, and in this way it did profoundly impress him.

“For the last two or three years he has been battling with the bodily disease to which he has now at last succumbed ; and, of course, his favorite studies and pursuits have been in a large measure interrupted, but his mind remained clear to the last. Only a day or two before he died he talked freely with me on the subject of the supernatural God and his own relations to a future life and the fitness of the ‘Christian scheme’ (as he termed it) to all the requirements of his nature, condition, and prospects. ‘I rest upon it,’ he said, ‘as a moral certainty from its adaptability to give firmness and courage to face the future. It is wholly agreeable to me, and my repose is unshaken. Whether I am about to die now or that event be yet some way off I know not, but I trust the issue without hesitation or fear to Him who disposes all things in His own good pleasure.’ ”

PUBLICATIONS OF J. HOMER LANE.

1. On the law of electric conduction in metals.
Am. Jour. Sci., 1846 (2), I, 230-241.
2. Notice of a novel mode of discharging a Leyden battery, with an explanation of its theory.
Am. Jour. Sci., 1849 (2), VII, 418-419.
3. On the law of the induction of an electrical current upon itself and of electrical discharges in straight wires.
Proc. Am. Assoc. Adv. Sci. Charleston, 1850, III, 359-361.
4. On the law of the induction of an electric current upon itself when developed in a straight prismatic conductor, and of discharges of machine electricity through straight wires.
Am. Jour. Sci., 1851 (2), XI, 17-35.
5. A visual method of effecting a precise automatic comparison of time between distant stations.
Am. Jour. Sci., 1860 (2), XXIX, 43-49.
6. On a mode of employing instantaneous photography as a means for the accurate determination of the path and velocity of a shooting star, with a view to the determination of its orbit.
Am. Jour. Sci., 1860 (2), XX, 42-45.
Jour. Photogr. Soc., 1860, VI, 302-304.
7. On the physical constitution of the sun (title only).
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8. Report to Mr. Hilgard of observations on the total solar eclipse as observed at Des Moines, Iowa, August 7, 1869 (dated August 28, 1869).
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9. On observations of the eclipse of the sun made at Des Moines, Iowa, August 7, 1869. A report, dated August 28, 1869, to the Superintendent of the United States Naval Observatory.
Published in Astr. & Met. Obs. U. S. Naval Observatory, 1867, Append. II, 165-173.
10. On the theoretical temperature of the sun, under the hypothesis of a gaseous mass maintaining its volume by its internal heat and depending on the laws of gases as known to terrestrial experiments.
Am. Jour. Sci., 1870 (2), L, 57-74.

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11. Description of a new form of mercurial horizon, in which the vibrations are speedily extinguished.

Proc. Am. Assoc. Adv. Sci., Troy, 1870, XIX, 59-61.

12. Report to Professor Benjamin Peirce, superintendent, on observations of the total solar eclipse observed at Catania, Sicily, on December 21, 1870.

Coast Survey Rep., 1870, 120-125.

13. Description of a new form of mercurial horizon [invented for the use of the Office of Weights and Measures, in the report of which its uses will be mentioned, but the description of the apparatus is given in the report of the Coast Survey].

Coast Survey Report, 1871, II, 181-192.

14. On the determination of the volume of a sphere (title only).

Proc. Nat. Acad. Sci., New York, October 30, 1873.

15. On the coefficient of expansion of the British standard yard, bar bronze No. 11, being a new discussion of the experiment of Sheepshanks and of Clarke.

Report C. and G. Survey, 1877, 148, 155-166.

(This is chapter IV of the report of J. E. Hilgard on the comparison of American and British standard yards, dated July 10, 1880, Append. XII, C. and G. Survey Report, 1877.)

MEMOIR
OF
WILLIAM FERREL.
1817-1891.

BY
CLEVELAND ABBE.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1892.

BIOGRAPHICAL MEMOIR OF WILLIAM FERREL.

We are assembled to think and speak of one who was known to very few by name or by face, and yet who has done more than any other single person to establish on firm foundations the mechanics of that branch of science which we call Meteorology. Since the days of Galileo men have accumulated observations of the temperature, humidity, pressure, and movement of the air for all parts of the globe. From these have been formed charts of the monthly and annual average features, and even tri-daily charts showing momentary conditions in the rapidly changing atmosphere. Innumerable studies have been based upon purely statistical methods, but whenever an attempt has been made to explain the causes of things and to trace out the interaction of the diverse forces the mind has recoiled from a strictly logical, deductive method, in view of the extreme complexity of the conditions, and has had recourse to crude hypotheses and approximations. All honor, therefore, to our colleague William Ferrel, who, animated by the conviction that no unknown or mysterious forces were present, attacked one problem after another and, by the logic of his thought and the invincible tenacity of his purpose, broke down the barriers of ignorance and opened the way for future explorers. But successful studies in meteorology could only have become possible to a mind disciplined by a long series of struggles with, and triumphs over, less complex problems: this in fact was the work of Ferrel's early life.

From his brother, Dr. Jacob Ferrel, of Kansas City, Missouri, we learn that "William Ferrel, the grandfather of our colleague, "came to America from the north of Ireland about the year 1785 "as a young man and soon married into an English family by the "name of Veach. Two children, a daughter and a son, were the "result of the union. The son was named Benjamin. His father "died when Benjamin was eleven months old. The family was "very poor, and Benjamin was put out for several years to be cared "for by strangers. When Benjamin grew up to years of maturity "he married into a German family by the name of Miller; to him "were born eight children, six boys and two girls, William being

"the oldest. William Ferrel was born in Bedford county, now
"Fulton county, Pennsylvania, January 29, 1817.

"At William's birthplace there is no 'town,' not even a house to
"mark the spot that gave him birth; nothing but a mound of clay,
"the remains of the old stick chimney that belonged to the round
"log cabin that was covered with clapboard. The place is known,
"not as the birthplace of the American meteorologist, but as 'Col-
"onel Knable's old saw-mill,' not a particle of which is now left to
"mark the place."

From the accounts that have reached me it is evident that Benjamin Ferrel and his wife were persons of sterling worth and much talent. The father of our colleague was a man of independent judgment and of great tenacity in defending his conclusions; he recognized the mental endowments of his eldest son, and so far as practicable contributed to his education. The faithful mother allowed no favoritism to influence her feelings, but treated all alike, held each in equal esteem, and received the equal devotion of all.

An autobiography, written out a few years ago by Ferrel at the request of a friend,* shows that from childhood his thoughts were given to the reasoning out of the causes that underlie surrounding phenomena. From his boyhood to his death William Ferrel was one who could not be satisfied with anything less than the most thorough attainable study of the subject that engaged his attention. From the beginning to the end of his life he was true to his own nature and lived up to his own principles and, by the steady prosecution of long trains of reasoning, succeeded in placing upon a surer foundation his own and our knowledge of many natural phenomena.

What a profound problem in psychology is suggested to us by the reflection that, in all ages of the world, so many persons are born who instinctively give themselves to searching out nature's laws and methods. The students of evolution have endeavored to demonstrate some few principles in regard to the inheritance of scientific ability, but who shall explain, in a direct and acceptable manner, the remarkable outcrop, here and there, of individuals whose ancestry gives no evidence of the possession of those qualities of mind that distinguish the brilliant descendant. The botanist singles out a peculiar flower or head of grain and calls it a *sport*, an exceptional individual among ten thousand of its comrades. He claims that the ancestor of that

* Mr. Alexander McAdie, of the Weather Bureau.

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particular plant was in every respect similar to the other plants, but that through some special influence attending the sprouting and growth of some one seed this sport has been produced. Consider this phenomenon in the material world and ask what corresponding power pervades the spiritual world, banishing intellectual sluggishness and directing our thoughts to the unseen causes of things?

Consider the problems in hydraulics, mechanics, and economics that must have occupied Ferrel's parents when in early married life their chief anxiety was to make a successful and profitable saw-mill. Those problems entered into the life of the child, and from his birth onward he was a mechanic in the highest sense of the word.

We may, then, not need to study the long line of ancestry which would lead us from Ferrel's birthplace in Bedford county, Pennsylvania, back to Ireland, England, and Germany. We have rather to assume that he inherited simply the sturdy enterprise, the obstinate perseverance that *will* conquer a success, the spirit that belongs to the pioneers of our young nation. We must turn to the material surroundings of Ferrel's childhood in order to study the development of his mind. Fortunately we are the better able to do this by means of his above-mentioned autobiography, from which it appears that before completing his elementary schooling he had, at the age of fourteen, been dividing his time between work in his father's saw-mill and work on his father's farm. Already, at this time, he was conscious of a special interest in the scientific items of the weekly newspaper and in the problems of his arithmetic, and was considered the best scholar among the boys of his age.

It was in the summer of 1832 that his boyish attention was arrested by the sight of a book on mensuration, with its geometrical diagrams, and soon afterward by the partial eclipse of the sun, which must have been visible where he then lived, near Martinsburg, West Virginia, from 7 a. m. to 8 a. m. on the morning of Friday July 27, 1832. Certainly few youths have ever, at the age of fifteen, been excited to study out the laws governing the occurrences of eclipses as was William Ferrel on this occasion. His account of the rapid progress made by him in co-ordinating the celestial phenomena is so unique that it must be commended to the admiration of all his friends. The result was that by the end of January, 1834, he had finished his own computations of predictions of the eclipses for the next year, 1835, all of which were remarkably well

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verified, considering that the only basis for these predictions was the numerical data obtained from old copies of farmers' almanacs, out of which he wove his own theory of the motions of the sun, moon, and earth.

The seventeenth celebration of his birthday, at the close of January, 1834, was marked by his first insight into a treatise on trigonometry and surveying, and although its study required a knowledge of geometry, which he had not yet acquired from books, and had to be prosecuted in the midst of every-day work on the farm, yet before the summer was ended his divine enthusiasm had overcome every obstacle. The work on these problems in surveying evidently suggested the idea of satisfying his unquenchable thirst for further knowledge by a visit to a famous land surveyor, an old Englishman, who lived some miles away in the mountains of West Virginia, from whom he obtained a copy of Simpson's Geometry. A year later, namely, in the winter of 1835-'36, two days were given up to a journey in midwinter to Hagerstown, Maryland, in hopes of purchasing Playfair's Geometry, the study of which, by the light of a blazing fire of lightwood or by the dim light of a tallow candle, occupied the rest of that year.

It was in the spring of 1837, at the age of twenty, that Ferrel first read of the law of gravitation and of the elliptical orbits of the planets and their satellites. It was still two years more before he had saved money enough to defray his expenses in a summer school of mathematics, at Marshall college, Mercersburg, Pennsylvania. Here, among other things, he for the first time saw a treatise on algebra and devoured it.

As Ferrel's course of study was a complete inversion of the ordinary methods of studying mathematics, I am led to remark that it is by no means evident that the order of studies generally pursued in our schools, namely, "arithmetic, algebra, plane-geometry, trigonometry," is that which is natural and easy for the youth or calculated to develop a useful efficiency in these acquirements. There is much to be said in favor of the early introduction into a boy's hands of the little treatise on geometry, by Herbert Spencer. It is very plain that Ferrel's passage from celestial motions and geometry through trigonometry and algebra up to the law of gravitation, and eventually to the differential calculus, was entirely in accord with the history of the progress of mathematics and with the needs of his own intellectual nature.

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Until Ferrel entered college his course of reading in mathematics was, as I have before hinted, the outcome of external circumstances. He had not been put into the hands of a professional trainer of the mind and forced to follow some conventional system, but as problems had presented themselves, whether suggested by the saw-mill or the eclipse or the surveying operations, he had worked away upon their solution by such methods as his own ingenuity invented. The same characteristic is found to pervade the greater portion of his subsequent career.

In justification of this we note that Ferrel's college education continued at irregular intervals from the spring of 1839, at Mercersburg, Pennsylvania, until after his graduation, in 1844, at Bethany college, West Virginia, after which, and as he has often told me, not knowing any better method of supporting himself, he followed the pioneer instincts and traditions of his family and settled at Liberty, not far from Kansas City, on the western edge of Missouri. In this frontier town he discovered one day, not displayed for sale, but put away on a back shelf, a copy of Newton's "*Principia*," which had been left in the village store by some wandering school-master as payment for an unsettled bill.

This marvelous accident (for who could expect to have found that priceless volume upon a western prairie?) saved Ferrel from being lost to science. During the succeeding three years, so far as his time and health permitted, he must have mastered Sir Isaac Newton's great work that stands at the portal of modern science. Only the logical Newtonian mind could grapple with the intricate problems that were about to enchain Ferrel's attention. He states that his studies of the tides began with Newton and suggested to him at once that the tides must tend to retard the earth's rotation.

It must have been about 1850, when teaching at Allensville, Todd county, Kentucky, that it happened, as he himself has told me, that he learned that some one by the name of Laplace had written something upon the subject of the "tides" which he desired very much to read. He therefore requested the country merchant to purchase him a copy when next he visited Philadelphia, for in those days, much more than now, western merchants made their annual journeys eastward by river, canal, and horseback to purchase their supplies. Bowditch's translation of Laplace's "*Mecanique Celeste*" proved a much bigger work than Ferrel had expected, but nothing daunted, in fact delighted to have this Thesaurus in his possession,

he set about conquering its difficulties. How completely he made both Newton and Laplace his own was shown in less than four years by his first published scientific paper on "the effect of the sun and moon upon the rotary motion of the earth" in Gould's *Astronomical Journal*. This memoir was dated at Allensville, Kentucky, September 24, 1853, and in it he for the first time gave a mathematical and numerical demonstration of the effect of the tides in retarding the rotation of the earth and the moon. Mayer had preceded Ferrel, but his memoirs were at that time unknown to the scientific world.

During the years 1854 to 1857 Ferrel lived at Nashville, Tennessee, where he established a private school. Here, by another fortunate providence, he added Airy's writings to his small but classic library and wrote his own second paper, also published in Gould's *Journal*, "On the problem of the tides with regard to oscillations of the second order," which was dated Nashville, May 24, 1856. Here also he wrote for his friend, Dr. W. K. Bowling, the popular essays that we have of late years republished, like monuments disinterred from the sands of Egypt, for modern students to admire. Our colleague now gave up teaching and welcomed the opportunity, brought to him through the kindness of Dr. B. A. Gould, to remove to Cambridge, Massachusetts, where he began that life of entire devotion to science which could alone satisfy his nature. During the thirty-five remaining years of his life his attention was principally given to the cognate problems of the motions of the ocean and of the atmosphere in connection with the rotation of the earth.

To have devoted so many years to one class of problems would seem to imply a very partial study of nature, a very one-sided development of the scientific life, but we can hardly say this of Ferrel. Indeed, every one who starts to investigate astronomy or physics or geology or biology must come back to the study of the atmosphere and the ocean, through the presence of which these various manifestations of nature have become possible. Ferrel was perfectly conscious that, by mastering the simpler phenomena relative to the movements of the ocean and the air, he was laying a solid foundation for all future progress in studying the past and future history of the globe and its inhabitants.

The preceding sketch shows that it is not improper to consider that Ferrel's courses of reading and study were largely suggested and controlled by circumstances outside of himself. Had not New-

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ton, Laplace, and Airy successively and accidentally come into his hands at the right time terrestrial physics would have waited for another master. It was a further interesting and accidental coincidence that while he was studying what these authors had to say the attention of the whole civilized world was fixed upon the brilliant experiments of Foucault, showing the rotation of the earth by his pendulum and gyroscope. The daily press teemed with scientific items, showing the awakened interest in the study of these subjects. Ferrel himself contributed, by his writings in the *Nashville Journal*, to the popular education on this subject.

The consideration of the important practical results attained by the study of the physical sciences delights us but little more than the study of the mental processes by means of which the student attains the solution of a problem.

Ferrel's habits of thought were pre-eminently of the deductive style. Twenty years of devotion to geometry and mechanics had thoroughly imbued his mind with all the consequences that must flow from that fundamental law of nature, "action and reaction are equal." He had acquired that additional seventh sense, the perception of force, to such a degree that he could quickly decide whether a proposed explanation was satisfactory or not.

Since the days of Sir Isaac Newton the importance of deductive theories has come to be more and more thoroughly appreciated in science. Based upon a few simple and well-established laws of force, the deductive theory may go astray through the intentional neglect of some residual quantity, and is therefore always checked by the appeal to experiment and observation. If observations prove wholly irreconcilable with theory, then it is probable that these neglected quantities or neglected laws of nature are disturbing the simple results of the fundamental principles originally considered. These latter, therefore, must be combined with the former. By this process a more complete theory is built up; every step of its development is deductive, and the final demonstration of its acceptability is found in the agreement of its results with observed facts. This was Ferrel's sole manner of work. He developed a theory of the gyroscope or rotascope, of vision and of the stereoscope; of the irregularity in the period of Algol; of radiation; and then experimented himself or collected data from others to test his theory. He filled out Newton's and Laplace's tidal theories and announced the resulting retardation of the earth's rotation, and then waited for facts to

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confirm it. He showed that, under any possible law of friction, Laplace's solution for the tides on an uniform ocean covering an ellipsoid of revolution must be erroneous, and then by studying the observed tides confirmed the corrected conclusion. He even dared to apply his knowledge of the tides to the most obscure problem that at present vexes terrestrial physics, namely, the magnetism of the earth, and to predict that the internal motions of a viscous earth would in some way contribute to the explanation of the phenomena.

We therefore recognize the same deductive style as pervading all his work in meteorology. It is not likely that a sudden flash of genius ever revealed to him the possible explanation of a perplexing fact. On the contrary, he was always mentally pursuing his deductive theories out to their remotest consequences and was looking for the phenomena which he knew ought to exist. If in any respect the analytical method was too difficult to give satisfactory results, he resorted to graphic methods, and, if need be, when these failed he deduced only in a general way the logical conclusions that followed from his premises, but in every case it was deduction checked by observation. One of his boldest essays in meteorological predictions was made in 1862, when, after analyzing what little we then knew of the meteorology and geography of the interior of Africa, he predicted that if ever we came to understand the subject more thoroughly we should find that the true cause of the annual inundations of the Nile lay not in the rainfall of the neighborhood of Lake Nyanza, but of a region ten to fifteen degrees north of that, where there must be some other large tributary.

A characteristic trait of Ferrel is illustrated by the evidence that many of his writings remained unpublished for a long time, until he could be persuaded that they were desired and needed, and most of his publications were drawn from him by the urgent requests of friends. His early papers of 1855 on "Vision," and again on "The Variable Star Algol," also his first essay on "Winds and Currents," and his subsequent paper on the "Motions of Fluids and Solids," were all prepared for publication in response to such requests. The paper on "Vision," having been published in a journal unknown to the physicists of Europe, has, I believe, hitherto not been mentioned even in the bibliographies of that subject. It does not even appear in the "Index Catalogue of the Surgeon-General's Library" under Ferrel's name, but will probably be quoted in the forthcoming vol-

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ume under "Vision." In this essay Ferrel worked out an original explanation of the mathematical and psychological processes by which we recognize the direction, distance, size, and shape of an object, and demonstrated its truth by experiments that suffice to give it a high rank among the theories that are worthy of the study of the oculist and psychologist. He also showed the errors of current theories in so far as he was acquainted with them, and applied his own views to the construction of the stereopticon and other pieces of apparatus.

The observation of the brightness of the variable stars is an important branch of astronomical work that has been specially furthered by Argelander and by our colleague, Dr. B. A. Gould, through his admirable astronomical journal. At Gould's request Ferrel wrote an appeal to astronomical amateurs in the Southern States, calling attention to the interest that attaches to this class of observations. Ferrel could not accept any facts of observation without attempting to gratify his inborn desire to explain the causes of phenomena; he therefore illustrated the interest that he felt in the subject of the variable stars by quoting Argelander's latest results and showing that the shortening periods of fluctuation of Algol must occur if the star is approaching the earth with a steadily accelerated velocity, and, inversely, that if it is receding from the earth the periods must lengthen. This, it will be noticed, is not an attempt to show that the brightness depends upon the distance, as was suggested by Hahn in 1798, but is a combination of Riccioli's hypothesis of 1651 as to the rotation of a spotted sun with Ferrel's own deduction from the known velocity of propagation of light. Fortunately in these days of spectroscopic analysis we are able to test the correctness of Ferrel's hypothesis, since if the star is approaching us while the periods are diminishing, and receding from us while they are increasing, then the displacement of the spectrum lines due to this motion will probably be measurable. The spectroscopic measurement of the approach or recession of Algol should agree with the velocity deduced by Ferrel from its photometric periods, and both combine to determine the dimensions of its elliptical orbit. Ferrel's hypothesis of 1855 has been abundantly verified by the recent work of Vogel in Berlin and Chandler in Cambridge.

It would be difficult in this short biographical sketch to do justice to the many items in regard to which Ferrel's studies have a

decided scientific importance. The smaller works, such as those on "Vision" and "Variable Stars," although but little known, lead us to logical conclusions that are eminently worthy of consideration. The greater works may be classified as those bearing, first, on the rotation of the earth; second, on the tidal problems; third, on meteorological questions.

Dating from his acquaintance with the works of Laplace, Ferrel began to be not merely an independent investigator, for that he had always been, but one who was generally in advance of the rest. He became a pioneer, "blazing" paths for the scientific progress of the age, and worthily wielded the implements he had so directly received from Newton and Laplace. His public scientific life-work began with studies upon "the tidal effect of the sun and moon upon the rotation of the earth," the similar effect of the earth on the ancient liquid moon, and, again, on the "influence of the tides in causing an apparent acceleration of the moon's mean motion." The problem of the rotation of the earth and the position of its polar axis had hitherto been studied on the assumption that we have to deal with a homogeneous, rigid body. It had been, as was thought, clearly demonstrated by Laplace that the inter-action between the earth and the atmosphere or the ocean could not affect the earth's rotation, much less terrestrial latitudes or longitudes. The clear perception of our colleague taught him that if there were such a thing as friction or any similar resistance within the ocean, then that must cause a gradual diminution of the rate of rotation, such that at the present time the day is somewhat longer than in former ages. This should produce an apparent acceleration, which is added to the real acceleration of the mean motion of the moon or, in fact, of any other celestial body. The independent demonstration of this principle by Delaunay was announced some time after the publication of Ferrel's paper of 1864, but the history of Ferrel's connection with this problem shows that he was in possession of this principle many years before. He himself has told us of his diffidence in presenting any paper whatever for publication and how, week after week for several months, he carried this written memoir in his pocket to successive meetings of the American Academy before he could summon courage to read it as his first contribution to that society.

The acceptance by astronomers of the general idea or principle that the earth's rotation could be affected by any appreciable inter-

action between its solid and liquid portions (in view of the fact that Laplace had considered it negligible) was as much of an epoch in physical astronomy as the subsequent works of Thomson on the "Tides on an Elastic Sphere" or of Darwin "On the Tides in a Viscous Globe." Undoubtedly there are some phenomena that must be attributed to the earth's plasticity and viscosity, and others that must be attributed to the earth's elasticity; but we have not yet fathomed all the phenomena that are characteristic of the earth viewed as a rigid solid with a fluid covering. The theory of the rotation of a rigid ellipsoid of revolution as it left the hands of Poisson in 1838 was crystallized by C. A. F. Peters in 1842 into the formulæ of his *Numerus Constans Nutationis*. Since then Peters, Clerk Maxwell, Gylden, Nyrén, and doubtless others, have extended the laws of rotation of a symmetrical, rigid earth, first, to Euler's case, when the axis of rotation does not agree with the principal axis of inertia, giving rise to the ten-month periodic variation of latitude; second, to cases in which, owing to internal disturbances a given, rigid earth suddenly or even slowly changes into another mechanically different rigid earth (by reason of some alteration in the distribution of its mass and therefore of its axes of inertia), in consequence of which other periodic changes of latitude are brought about. All these studies assume a nearly symmetrical ellipsoid of revolution whose figure is determined by the general distribution of terrestrial gravity. From some notes that have come into my hands I infer that Ferrel had undertaken the collection of data preparatory to the solution of the question: "What is the effect upon the rotation of the earth, considered as a rigid ellipsoid, of the attraction of the sun and moon upon continents and oceans, considered as rigid masses irregularly superposed upon the assumed symmetrical ellipsoid?"

I have elsewhere indicated my belief that if he had lived to finish this study he would have elucidated two problems that are at present actively engaging the attention of the world. He would, namely, have shown that on the one hand these great superficial irregularities produce complex, periodic terms to be added to the ordinary formulæ for precession and nutation which we now begin to recognize as changes in latitude. On the other hand, the immense forces involved in the regular phenomena of precession and nutation, as well in these irregular changes in latitude, produce a system of strains throughout the earth by virtue of which the strata are alter-

nately compressed and stretched. Sometimes these strains are sufficient to cause fractures, but they are always sufficient to develop the phenomenon known as piezo-electricity, and these piezo-electric currents are the ultimate cause of the electrified condition of the globe and its atmosphere and in part of its terrestrial magnetism.

These studies into the action of the sun and moon upon the rotation of a rigid globe do not in any way render unnecessary the profound investigations into the phenomena attending the rotation of an elastic earth or of a viscous globe. The earth is really both rigid and elastic and viscous, for these three terms do not relate to properties of matter *per se*, but relate to the phenomena exhibited by that matter when acted upon by exterior forces during short or long periods of time. Our globe, whirling rapidly around, carrying a perturbing mass on its surface, is by the latter subjected to a daily strain, both radial, longitudinal, and latitudinal. It is the repetition of this strain, day after day, and especially its periodic increase with conjunctions of the sun and moon, that eventually produces appreciable effects.

We cannot too deeply regret that Ferrel was not spared to finish the discussion of these problems upon which he had already thought and written in his "Tidal Researches" in 1874. The last paragraph of this great work gives us the first published rational basis for the so-called Halley's theory of the secular changes in the earth's magnetism. Although it is probable that this theory is far from representing nature, yet it is very indicative of the wide range and boldness of Ferrel's thoughts to find that he has been logically led to dwell upon it.

The second general field of work in which Ferrel engaged was the study of the tides as such. This subject he developed, both analytically and numerically, and finally even mechanically, so that his machine for the mechanical prediction of the maxima and the minima of the tide remains to this day in constant use at the office of the Coast and Geodetic Survey. This apparatus was devised quite independently of the harmonic analyzer of Sir William Thomson and fulfills a different purpose, although in some respects they have similar features. The date of its actual invention does not appear from Ferrel's papers, but merely the statement that the plan in its final shape was submitted to the Superintendent of the Coast Survey in 1880. I happen to know, however, that already in 1861, when I was in daily personal intercourse with Ferrel in Cambridge

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and when he was earning a scant livelihood by computing lunar and planetary ephemerides for the Nautical Almanac, he was even then speculating on some combination of wheels that should save all this unnecessary mechanical computation, so that I presume the idea of the computing machine for calculation of ephemerides was an early inception, and that its growth in his own mind was gradual until the times became favorable for the actual realization of a similar tidal machine.

Ferrel had occasion in 1856, and again in his "Tidal Researches" of 1874, and in the *Philosophical Magazine* of 1876, to announce his own independent personal conviction in reference to a point that has given rise to much discussion, namely, the proper determination and interpretation of certain arbitrary constants that appear in the development of the tidal function into a series of periodic terms. His views were opposed to those expressed by Sir William Thomson, but Ferrel's letter of November 17, 1875, as printed in the *Philosophical Magazine* of February, 1876, could not have been drawn from him easily, so averse was he to any appearance of controversy. He had not the least desire to pose before the world as a wrangler over scientific points on which his own convictions were clear and settled. Nothing better illustrates Ferrel's innate shyness than the fact that both in 1876 and again in 1884, when Sir William Thomson (now Lord Kelvin) visited Washington nothing could induce him to voluntarily call upon one for whom he had such profound respect and yet with whom he had had occasion to differ so decidedly, for fear lest some constraint might be felt by either one during such a meeting. However, a meeting was brought about in 1884, without his connivance, and, to Ferrel's profound delight, the conversation was as unconstrained as could be desired. In the last year of his life, having perceived that his views as to the errors of Laplace and Thomson were still unappreciated in England, Ferrel returned to this subject, and in two papers in *Gould's Astronomical Journal* for 1889 and 1890 showed, as it would seem, with conclusive clearness that in the presence of frictional resistances and for oceans of finite depths any initial tidal motion would be gradually annulled, and that practically no tides are observed by us except those that are directly maintained by the sun and moon, or the so-called forced tides as distinguished from free tidal waves due to possible initial motions. On the other hand, Ferrel showed that if no friction exists, even then the free tidal waves disappear, ex-

cept in the case of an ocean of a certain critical depth such that the waves may coincide in period with the forced diurnal and semi-diurnal tides, in which case the tides may, of course, have any value whatever.

The third class of Ferrel's important works relates to the subject of meteorology. It is probable that his attention to the subject of storms as such was, in a general way, excited early in life by the wide-spread dissemination of Espy's theories (see *Popular Science Monthly*, April, 1889, xxxiv, p. 834). These latter were gradually developed during the interval from 1820 to 1850, and Espy's delightful lectures throughout the country, as well as the heated controversies maintained by his opponents, must have been known to Ferrel.

But Ferrel expressly tells us that his active interest in the problems relating to the general circulation of the atmosphere was more especially developed by reading Maury's "Physical Geography of the Sea." (Ferrel refers apparently to the second edition, dated December, 1854, and published in 1855. His own article was published in October, 1856.)

Ferrel's first meteorological essay utilizes a characteristic, discreet selection of well-established facts taken from Espy's third report, from Redfield's writings, and Maury's "Physical Geography of the Sea." He explicitly rejects numerous hypothetical explanations which are inappropriate and unsatisfactory, and shows that the general laws of winds, pressures, and storms are deducible from a few simple principles. In those days it required close study and a very clear insight into the subject and a very independent mind to prevent one from becoming a partisan either of Maury or of Espy or of Redfield, not to mention Dr. Hare, the electrician of Philadelphia, or Dove, the statistician of Berlin. Precisely such a mind we find in Ferrel, who, from his surroundings in Nashville, was enabled to look out upon the world of meteorological disputants and to insist upon logic and reason in place of a war of words. In reading this clearly written essay we realize that his perception of the relative magnitude of the forces involved enabled him to select the more important results of meteorological observations and to combine these with laws of nature that his own researches convinced him were equally important, although hitherto hidden from scientific vision. We may in later years, with Ferrel's help, have in some details progressed beyond his early essay of 1856 on "The winds of

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the atmosphere and the currents of the ocean," but in all its important features it still stands as the first satisfactory general explanation of the atmospheric and oceanic motions based upon a proper consideration of the effects of density or buoyancy in producing currents and of the rotation of the earth in modifying these when once started. This essay, which explains not only the winds, upper currents, and hurricanes, but the distribution of barometric pressure, depending upon the winds, although it is free from mathematical formulæ, is yet based at every step upon the formulæ that he subsequently published. This memoir was practically unknown to meteorologists until inserted in the collection of popular essays which I prepared as "Professional Paper No. XII of the Signal Service." A similar unfortunate fate seems to have befallen Ferrel's mathematical paper of 1858 and its special pamphlet edition of 1860, since the "Mathematical Monthly," like the "Nashville Journal of Medecine and Surgery," were not periodicals likely to circulate among meteorologists. Although the special edition of 1860 was originally sent by Ferrel to those who he thought would be interested in it, yet I believe the first published reference to this work is to be found in appreciative notes introduced by Professor Everett into his translation of Deschanel's Philosophy, ten years afterwards. Since that date Archibald, in England, and Sprung, in Germany, have contributed to give a further impulse to the effort to make Ferrel's researches more widely known, and at the time of his death it could safely be said that he had been recognized throughout the world as having given to the science of meteorology a foundation in mechanics as solid as that which Newton laid for astronomy.

When I came to the assistance of the Signal Office in January, 1871, it was my first care on the one hand to apply Ferrel's methods of looking at the atmospheric motions to the maps and predictions that were made by me daily. In a little pamphlet written in April, 1871, I quoted the general results of his studies, so as to make them more widely known, but especially was I solicitous to secure his own further attention to the meteorological problems that were now daily before us. He was, indeed, at that time too much engrossed in his tidal researches to allow of an immediate return to meteorology, but a few short papers in 1871 and 1874 show that he did not neglect to read and study upon this subject. His principal thought between the years 1867 and 1875 was undoubtedly given to the tides. At the close of these tidal studies and while engaged in the construction

of his tide-predicting machine he took up a new work for the Coast Survey entitled "Meteorological Researches for the use of the Coast Pilot." This work was published in three parts as appendices to the successive Coast Survey reports for the years 1875, 1878, and 1881, but the actual dates of finishing and publishing these appendices were 1878, 1880, and 1882 respectively. The composition of the first of these three must have been in the year 1875.

We may therefore consider that Ferrel's work in meteorology was renewed in 1875, and continued unabated during his employment by the Coast and Geodetic Survey and subsequently while attached to the Signal Office under General Hazen, from August, 1882, until his resignation, in September, 1886.

Problems in tides and currents, rotation of the globe, flow of the atmosphere, even those in terrestrial magnetism and gravity, or internal heat and viscosity, are all apparently correlated in such a way that the same mathematical formulæ are everywhere being applied to certain modifications of the same physical principles. The analysis that applies to hydro-dynamics also applies to electricity, and will eventually resolve the problems of elasticity. Therefore I say that Ferrel, having once mastered the methods founded by Newton, Laplace, and Airy, found in the atmosphere a further field for the application of these.

We may rapidly review a few of the problems that Ferrel has elucidated in meteorology. His attention was first drawn to the remarkable fact that the barometric pressure is less in the equatorial zone than at a greater distance from the equator, whereas near the poles it is again low. This phenomenon he deduced as a simple result of the theorem that every motion along the surface of the earth is accompanied by a deflection to the right or a tendency to such deflection in the northern hemisphere, but to the left in the southern hemisphere. This general proposition had, unknown to Ferrel, been announced by Poisson and may have been known to Laplace—it was even known to Tracy, of New York; but, ignored and forgotten by all the world, it became in Ferrel's hands the key to several unexplained phenomena and is now quite frequently known as "Ferrel's law." From this law Ferrel first deduced the relation between the winds and the barometric gradients and gave us the means of determining what is called the coefficient of resistance of the surface of the earth to the winds. He was thus able to compute approximately the general velocity and

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character of the currents in the upper portion of the atmosphere inaccessible to observation by means of clouds or even balloons, and showed what intimate relations there must be between these upper currents and the development and motions of general storms. He demonstrates the true cause of the low barometer about the south pole and the lesser depression at the north pole as resulting from the deflecting action of the revolving earth. He shows why the pressure is low at the equator and especially why the barometric oscillations are so regular in that region. He explains how it is that revolving hurricanes have a tendency to move toward the poles on account of internal moving forces, aided further by the distribution of aqueous vapor and the use of warm buoyant air in advance. Inversely anti-cyclones must move from the poles. He shows the mechanical cause that underlies the formation of the belts of calms near the tropics. All this was in or before 1860.

When Ferrel again took up meteorology, in 1875, he was thoroughly imbued with the development that Thomson, Peslin, Reye, and Hann had given to Espy's condensation theory. He elaborated the study of the phenomena of cyclones and anti-cyclones in an elastic moist atmosphere subject to friction and applied the theory to the formation of thunderstorms and tornadoes as well as to extensive hurricanes. Concerning this work meteorologists throughout the world may well adopt the language of Prof. Wm. M. Davis: "The student may search the literature of the science through and through; he will find nowhere else any adequate consideration of the cause of the terrific blast of the tornado. He may wander from one unsatisfying theory to another as the doubter wanders from creed to creed, finding no rest for his unhappy disbelief until at last he reaches the true faith, on which he rests with confidence and comfort."

There is one feature of Ferrel's work on the origin and maintenance of storms that was overlooked or misapprehended by most of us until attention was called to it a few months before his death, and undoubtedly our imperfect apprehension of his own clear conceptions troubled him not a little during the sickness that clouded the last year of his life.

So fully had he developed the action of the latent heat of condensing vapor that we forgot that he had in mind any other possible source of disturbance, so that when Hann stated the apparent insufficiency of this heat, and when Siemens, Oberbeck, and possi-

bly others stated that apparently the general circulation of the atmosphere by generating large whirls may initiate a cyclone, or when Helmholtz showed that the trough of an atmospheric wave may give rise to expansions, coolings, and clouds, we all welcomed the suggestion as apparently helping us out of certain difficulties. But it now seems that Ferrel had long since investigated the effect of the general circulation and had laid it aside as of minor importance before devoting himself to the development of Espy's theory. He had shown its efficiency in producing the antarctic whirl, but its unimportance in ordinary cyclones. It may be that with a better knowledge of the mechanics of the atmosphere we shall eventually be better able to show to what extent our storms depend upon moisture, and to what extent on the general circulation of the air, and to what extent on the local circulations, but it is not likely that we shall be able to overturn Ferrel's formula expressing the relation between the wind and the barometric gradient, and which shows that the general circulation can only produce very moderate high pressures and low pressures, and that it needs to be supplemented by the heat of condensing vapor before we can get at the explanation of our storms. A slight fall of pressure, due to the general circulation, may decide as to the time and place where a storm is to originate, but cannot explain its subsequent development. Even the radiation of heat by the air, about which we have lately learned a little, is mentioned by Ferrel as a factor to be considered in studying areas of high barometer and cold air, but of its numerical value he had no exact data and classed it with other matters secondary to the aqueous vapor that he was studying.

Intimately connected with the origin and maintenance of the storm is the question of its progressive path from day to day. On this matter Ferrel maintains that the motions of the cyclones must be principally controlled by the general circulation of the atmosphere, which will carry them westward within the tropics, but eastward in the temperate zones. He had, indeed, shown that there is a slight excess of pressure within a cyclone, tending to push it away from the equator, and others had found that the distribution of temperature or of rain seemed to control the motions; but Ferrel concludes that all these are of less importance in comparison with the general circulation of the atmosphere. By this latter expression I understand that he includes, not merely the translatory motion, but the general whirling motions of the atmosphere so far as

they depend on the difference of density of the air in the warm equatorial and the cold polar regions.

The motion is similar to that of a river in which, when we see little whirls traveling down stream, we say they are carried by the general current down stream as though the whirl had no influence on the stream; but when that same river swells to a mighty rush of water, in which enormous whirls occur, we think of these whirls as the characteristic feature of the river and as having so great an influence upon the flow of water that we can hardly bring ourselves to speak of the whirls as being carried along *by* the general current of the stream, but would rather say they are carried along *in* the general current of which they constitute an integral part. In this latter way Ferrel thinks of the general circulation of the air from the equator to the poles. Hurricanes, thunderstorms, and tornadoes, cold waves and high pressures, all form parts of this general circulation, and the cyclonic eddies are said to be carried along *in* it or *with* it rather than *by* it. It does not appear that Ferrel has neglected to consider these important factors. The so-called "new views" had all been considered by him twenty years ago. It was not his habit to publish his explanation of a given phenomenon until he had also tested all other explanations of all its important features. His mechanics of the general currents of the air and of the motions within cyclones and tornadoes will undoubtedly stand the criticisms of future times as safely as will the results of his studies on the tides of the ocean and the rotation of the earth.

The last ten years of Ferrel's life were years of great productivity in the development of his views on meteorology. His larger works were his "Meteorological Researches," his "Recent Advances," his unpublished course of lectures, and his "Popular Treatise on the Winds;" but his minor works were also important, such as his "Psychrometric Formulæ and Tables," his "Barometric Hypsometry" and the allied "Method of Reduction to Sea-level," his studies on the "Arago-Davy Actinometer" and on the "Law of Thermal Radiation."

In Ferrel's every-day life there seemed to all who knew him to be a certain amount of isolation too large to satisfy us, and he himself felt this. Preoccupied by problems, and therefore necessarily alone with his own thoughts, he had, even from boyhood, neglected the development of the social side of his nature. It was only when

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we searched him out and interrupted his studies that we found him kind and helpful and appreciative of human intercourse. It is only after we have lost him that we fully realize the privilege that we have enjoyed, now these thirty years past, of appealing to him at any time.

Comparatively few found opportunity to intrude upon the uniform simplicity and retirement of his life. Unmarried, and generally far from his relatives, he passed his days in a systematic regularity from which he knew that he must not depart if he were to master the problems of the ocean and the air. A half hour daily in the parlor of the friends with whom he lived, or a short call upon others whom he held in esteem, broke up the steady current of his thoughts sufficiently to keep him in good condition for mental work.

Much affection and care did he bestow upon his near relatives. He seems to have been very sensitive to the claims of kinship. Twenty-five years of faithful service were first given to the family homestead; then followed fifteen years of study and teaching in the West; after this he resigned himself entirely to the promotion of the sciences, a study to which he brought those habits of steady and intense mental work that he had learned at home.

From 1856 to 1886 thirty years of hard work well done passed smoothly by. He then voluntarily retired from official life, thinking to live quietly with his brothers and sisters. Five years were thus spent. Beloved by them and appreciated by all, he expired at their home, at Maywood, Kansas, September 18, 1891, and lies buried in the cemetery of that town.

Monuments are often erected to the memory of distinguished scientists, but I know of none that would be more appropriate to Ferrel than a complete edition of his scattered publications by which to perpetuate his name and influence among the philosophers of the future.

AUTOBIOGRAPHICAL SKETCH.

(Written in January, 1888, by William Ferrel at the request of his friend, Mr. Alexander McAdie.)

I was born of humble parentage, in Bedford (now Fulton) county, Pennsylvania, on the 29th of January, 1817. My father's father was of Irish descent, as indicated by the name; his mother was English. My mother was of entirely German descent. I commenced going to school at the usual age, to the schools of the neighborhood, which, however, were of a very inferior order, the teachers mostly being able to teach only reading and writing and a part of the arithmetics. It was thought that I made very rapid progress, and I soon had the reputation of being the best scholar in the neighborhood.

While we lived in Pennsylvania my father was interested in the lumber business and a saw-mill.

When I was twelve years of age he bought a farm in Berkeley county, Virginia (now West Virginia), to which we moved in the spring of 1829. Although we moved entirely across the State of Maryland (here only three miles wide), the whole distance was only fifteen miles.

I was now kept closely at work on the farm, but I went to school here two winters and completed my common-school education.

The school-house was an average one of the country at that time—a rude log cabin with oiled white paper instead of glass for window panes. My last teacher took me through arithmetic and the English grammar.

Having now mastered my school books, I had nothing to engage my mind in the way of study and nothing to read except the weekly Virginia Republican, published at Martinsburg. This I was always very anxious to see, and could scarcely wait for its coming to hand, mostly for the scientific items in it which I occasionally found.

About this time I saw somewhere a copy of Park's Arithmetic,

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with a short sketch of mensuration at the close. At the sight of the diagrams I was at once fired with an intense desire to have the book; but I had no money, and at this time I was too diffident to ask my father for even a half dollar or to let him know that I wanted the book. Some time afterward I earned fifty cents in the harvest field of one of our neighbors, and with this I determined to buy the book. The first time I had a chance to go to Martinsburg I inquired at a store for the book, but the price was sixty-two and a half cents. I told the storekeeper I had only fifty cents, and so he let me have it at that price. It was a light task to learn all that was in it.

On the forenoon of the 29th of July, 1832, as I was going to the field to work, I observed that the sun was eclipsed. I had not known that such an event was going to occur. This excited me and set me to thinking. I had not read or thought anything about astronomical subjects before, but knew somehow that an eclipse of the sun was caused by the moon's passing between it and the earth, and that the lunar eclipses were caused by the moon's passing through the earth's shadow. I had nothing to aid me in the study but one German calendar, such as farmers use, and a copy of Adams' Geography, in the appendix of which there were solutions of problems on the globes. From these latter I learned the relation between the equator and the ecliptic and the signs of the zodiac. In the former I traced the motions of the sun and moon. The times of the sun's entering the equinoxes and solstices were accurately given; for the other signs of the zodiac only the day was given. I discovered that the motion of both sun and moon in their orbits was irregular, and that it was greatest and least at opposite points.

My theory was that the earth and moon move with uniform velocity in circular orbits, and that those orbits were eccentrically situated with regard to the sun and earth. With regard to the moon's path, I knew that it crossed the ecliptic, but I did not know at what angle, and I also at first supposed that the node was fixed. At the beginning of the next year, when the next calendar came to hand, I discovered from the predicted eclipses that it must recede. I saw from the calendars that there was some cycle of nineteen years, and suspected that this had something to do with the moon's node. This would make the node recede about nineteen degrees in a year, and this was what the next year's eclipses seemed to require.

I now found calendars in the neighborhood for a few of the preceding years and studied the eclipses and traced the motions of the sun and moon through all of them. I now commenced to make out a solar ephemeris, not from my theory, however, for my mathematics then were not adequate, but from the calendars. Except for the four points named, the time to the nearest day only was given for the times of the sun's entering the signs of the zodiac; but, taking advantage of the shifting caused by the leap-years, I was enabled to fix these times within one-eighth of a day, or the places within 7' of an arc. I thus made out my rude ephemeris for one of the four years and then made the proper allowances for change of place for the other years, corresponding to the beginning or middle of the day.

In the calendars the places of the moon each day were given to the nearest degree. In tracing the moon's motions I perceived that the angular velocity was not only different in different parts of its orbit, but that it did not perform complete revolutions in equal times, occasioned by the changes of the places of perigee and apogee, which were given in the calendars. I perceived, however, that it performed fifteen revolutions and forty-two degrees over in very nearly equal times (very nearly 413 days), and this corresponds approximately with fourteen conjunctions of the sun and moon. Commencing, therefore, with some conjunction or opposition of the sun and moon, new moon and full moon, and counting forward 413 days and a little over, the exact period having been determined, and knowing the moon's position at conjunction from my rude solar ephemeris, I could locate the moon in its orbit at the end of that time and also, from my solar ephemeris again, the sun's position at that time. This brought the moon within about six degrees, a little more or less according to the season of the year, of a conjunction with or opposition to the sun according as I started from the one or the other. For these few degrees I could compute pretty accurately the time required for the moon to pass over them, taking into account its varying velocity with its different relations to perigee or apogee, and also determine the time when the moon would overtake the sun, or, in other words, the time of new or full moon. Knowing the places of the moon's nodes and consequently the time of the sun's being near them, I could go back to some new or full moon the year before, which was about 413 days before, and compute the new and full moons at or near the times of the sun's being at the nodes and so determine the central times of the eclipses. I

had to determine the angle of inclination of the moon's orbit to the ecliptic and also the places of the moon's node, allowing an annual recession of 19° , which would best satisfy the eclipses as given in the calendars for four or five years which I had. In doing this I thoughtlessly assumed that the earth's shadow at the distance of the moon had the same diameter as the earth. Upon this assumption I spent a vast amount of time in determining these elements, but could get no positions of the nodes or inclination of the orbit which would satisfy the eclipses. The amount of study I gave to the subject, both day and night, was very great, but I at last gave the matter up in despair.

Some time after I was at work one day, toward evening, on the threshing-floor and saw the shadow of a distant vertical plank against the wall and observed that it was much smaller than the width of the plank, and the reason of it occurred to me at once. I then saw the error of my assumption with regard to the earth's shadow in my eclipse investigations and was now very anxious to go over again all my computations with the true diameter of the earth's shadow, for, knowing the distance of the moon and the angular diameter of the sun, I was able to determine this. As soon as I could find time I went over the whole work, and everything came out as satisfactorily as could reasonably be expected with my methods. I put the angle between the moon's orbit and the ecliptic at 5° , this seeming to be the value which satisfied best the conditions of the eclipses. This was in the winter of the first part of the year 1834. I now ventured to predict by my method the eclipses for the next year, 1835. I determined that there would be three eclipses—two of the moon and one of the sun. I computed the times of the middle of the lunar eclipses, the relations of the moon's path to the shadow and duration of the eclipses. In the case of the solar eclipse I merely predicted the time of the sun's being centrally eclipsed on the meridian. I made a record of the whole in a book and waited for the next calendar for comparison with its predictions. All the circumstances of the lunar eclipses agreed remarkably well, and the greatest error in the predicted times was only nine minutes.

I had heard a neighboring youth, a little older than myself, speak of a book that he had seen with a great many diagrams in it and which he said was called trigonometry. Hearing of the diagrams I was very desirous of obtaining it, and so the first time I had occasion to go to Martinsburg, after finishing my eclipse computa-

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tions, I inquired for a treatise on trigonometry. It happened they had on hand (in an ordinary store) a copy of Gummere's Surveying, which I bought. Although it was not exactly what I called for, yet it had a good deal of trigonometry in it and suited me better than what I called for would have done.

This was in February of 1834. During the balance of the winter and the spring I completely mastered it, working out all the examples except a number of miscellaneous examples at the close of the work, for which no rules had been given and which required a knowledge of geometry. During the summer, as I had a little time to spare, I dwelt upon these, giving weeks sometimes to a single proposition. It happened that during the summer I was engaged a great part of my time on the threshing-floor, which had large doors at both ends, with wide and soft poplar planks. Upon these I made diagrams, describing circles with the prongs of the pitchfork and drawing lines with one of the prongs and a small piece of board. One by one I mastered in this way these problems, all except three. For more than a quarter of a century these diagrams were visible on these doors, and in returning occasionally to the old homestead I always went to take a look at them.

In studying Gummere's Surveying I found many references to Playfair's Geometry. Late in the fall of 1834 I went to see a famous land surveyor, who lived in the mountains, to borrow the Geometry, if he had it. He was an Englishman and then an old man, and though he had a number of mathematical books they were all old authors. He did not have Playfair's, but he loaned me Simpson's, which I studied during the winters of 1834-'35.

The next winter, 1835-'36, I rode twenty miles in midwinter to Hagerstown, Maryland, to get Playfair's Geometry. It required two days to make the trip. This I studied during the balance of the winter and afterward occasionally, when I had any spare time. In all my winter evening studies I had mostly the light only of a blazing fire, brightened up frequently by throwing into it a piece of lightwood; at other times I had the dim light of a tallow candle.

Some time before this I found a copy of Hutton's Mensuration in the neighborhood, which I borrowed and studied.

In the spring of 1837 I found in Martinsburg a very elementary work on Natural Philosophy, treated in the manner of a dialogue. From this I first learned the law of gravitation, and that the moon and planets move in elliptical orbits.

In the spring of 1839, having saved a little money made by teaching, I resolved to go to Marshall College, Mercersburg, Pennsylvania, to spend the summer in the study of mathematics. The college had a winter and summer session, with a vacation spring and fall. I entered the preparatory school connected with the college and first took up the studies of algebra, geometry, and trigonometry. I now saw for the first time a treatise on algebra. It required only a small part of my time to keep along with the classes, and so I commenced to study the Latin and Greek grammars, mostly for the sake of having something to occupy my time, for I did not know that I would spend here more than one session. In the fall I returned to Virginia and taught the next winter, and also studied my Latin and Greek grammars and my researches. The next spring, with the promise of some pecuniary aid from my father, I returned again to Mercersburg. I now continued my Latin and Greek in the preparatory school, but entered the college classes in mathematics. By the rules of the institution irregular students who did not contemplate a full course could enter any of the classes for which they were prepared. At the close of this session, with the progress which I made during the session and the preceding winter while teaching and in consideration of my standing in mathematics, I was admitted to the sophomore class, though I was allowed to continue my mathematics with the juniors and seniors. During this year Professor Budd formed a volunteer class of such juniors and seniors as were willing to take up the study of certain branches not contained in the regular course. We recited every Saturday morning. He occasionally gave problems to the class for solution during the week, not contained in the text books.

On one occasion he gave the problem: Given the distance of a well from the three angles of an isosceles triangle to determine the triangle. I solved the problem and gave the solution to a few others of the class, but most of them had no solution. Professor Budd, after inquiries from the class, found that I was the only one who had succeeded in solving it. He said he had given it to several classes before, but had never obtained a solution. It was easy to me at the time, for it was one of the problems which I had solved while at work on the threshing-floor, with the use of diagrams on the barn doors, long before I had seen a college and algebra or geometry, except Simpson's Geometry, which I obtained the next winter.

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At the commencement of the college, in the fall of 1841, I had been admitted to the junior class, but I had completed the full college course in mathematics and also much not in the course in the volunteer class.

My money was now exhausted, and I left with no intention of ever returning. I returned to Virginia and taught the next two years. Meanwhile a new college had been gotten up in Virginia, Bethany College, and I resolved to return to it and spend a year at least. Being in my own State and under the control of the "Christian Church," of which most of our family were members, was the only reason why I returned to this instead of Marshall College. Reverend Alexander Campbell was at the head of the college. I was admitted to the senior class and was one of the first graduating class of the college. This was on the 4th of July, 1844.

In the fall of this year I went west and settled in Liberty, Missouri, where I taught nearly eighteen months, but was obliged to give up my school before the last session closed on account of my health.

While in Liberty I found in a store a copy of Newton's "Principia." It had been ordered by an itinerant, who said he was about to open a school there, but before it, and a large number of other books which he had ordered arrived, he had disappeared. The store-keeper was glad to get five dollars for the work, less than half its first cost in Philadelphia. I now entered upon the study of this, but had not made much progress before my health failed. In the spring of 1846 I left Liberty with the intention of returning to Virginia. At St. Louis, on the way, I called on Professor Stewart, who had been my instructor in mathematics at Bethany College, but who had resigned and now had a private school in St. Louis. He persuaded me to go to Clarksville, Tennessee, and the country around about and gave me a letter to a friend of his at Clarksville. After calling on him and spending a week there I went up into Todd county, Kentucky, just over the State line and only fifteen miles from Clarksville. I here commenced teaching a small school, but large enough for the condition of my health. This was a fine part of the country and there was considerable wealth. Most of the labor was slave labor. I remained in this county about seven years, teaching first at Hadensville and then at Allensville; both were villages. Soon as my health admitted I took up Newton's "Principia," and I now became first interested in the tides and conceived the idea

that the action of the moon and sun upon the tides must have a tendency to retard the earth's rotation on its axis. Knowing that Laplace had treated the subject extensively in the "*Mecanique Celeste*" I was very desirous of obtaining a copy, mostly to see what he had on the subject. I accordingly instructed a village merchant, on going to Philadelphia for a supply of goods, to procure me the work, having little idea of the magnitude of the work or the cost. On learning the cost at Philadelphia he did not procure it for me until after writing to me and hearing further from me. I had now plenty to study, in connection with my teaching, for several years. In the summer of 1853 I wrote my first scientific paper for publication in Gould's *Astronomical Journal*, Cambridge, Massachusetts, "On the effect of the sun and moon upon the rotary motion of the earth." I showed that, upon a certain reasonable hypothesis, it would be very sensible; but as no retardation had been observed except what was accounted for in another way I concluded the effect was counteracted by the earth's gradual contraction. It was a carrying out the idea first conceived on reading Newton's "*Principia*." I had, however, been anticipated in the fundamental idea by Mayer, but I was the first to treat the subject in a completely mathematical manner, and it was entirely original with me.

In the spring of 1854 I located in Nashville, Tennessee, and opened a private school. Prof. W. K. Bowling, M. D., of the medical college here, became a warm friend of mine. He published the *Nashville Medical Journal*. He was desirous of giving it a general scientific character and desired that I should contribute something on almost any scientific subject. I accordingly contributed several essays on vision, including the principle of the stereoscope. While in Nashville I found in a book store a volume containing Airy's "*Figure of the Earth*" and his "*Tides and Waves*."

I studied these in connection with Laplace, especially the part with regard to the tides. I now furnished a second paper for Gould's journal, "The problem of the tides with regard to oscillations of the second kind." In this paper I showed that Laplace's celebrated result that the diurnal tide in an ocean of uniform depth would vanish, is simply one of an infinite number which would satisfy the differential equations of condition, in the case of no friction, which was the case treated, each depending upon the initial motions of the water, but that in the case of the least friction these

motions would gradually vanish and those only would be left depending directly upon the acting forces. In my result there were terms depending upon an arbitrary constant, and I showed that this constant could have a value which would make the diurnal tide vanish. The terms depending upon this constant were entirely independent of the forces and, in the case of nature with friction, would vanish and the terms only would be left depending upon the forces. The terms depending upon the arbitrary constant depended simply upon the initial motions which the water might be supposed to have at the start.

At another time I found in the same store Maury's "Physical Geography of the Sea." This first turned my attention to meteorology. From this I learned that the atmospheric pressure was greatest near the parallels of 30° and less at the equator and in the polar regions, and I at once commenced to study the cause of it. In my study of the tidal theory I saw that there were certain terms in the equations depending upon the earth's rotation which affected the tides, and that there must be similar terms in the complete equations of the atmospheric motions and pressures.

In conversation one day with my friend doctor Bowling I told him I had read Maury's book, and he at once was desirous of knowing what I thought of it. I told him that I did not agree with Maury in many things. He then desired me to "pitch into him," as he expressed it, and furnish a review for his journal. This I declined to do, but at length consented to furnish an essay on certain subjects treated in the book and notice Maury's views a little in an incidental way. This was the origin of my "Essay on the Winds and the Currents of the Ocean," published in the *Nashville Journal of Medicine* and republished by the Signal Service as Professional Paper No. XII. About this time there was considerable discussion with regard to the gyroscope. I commenced to look up that subject and prepared a paper for doctor Bowling's journal on "The Gyroscope." From my researches I concluded that if there was an arm extending back from the pivot upon which the instrument revolved, on the opposite side and a weight were attached a little heavier than the other side, the gyroscope would gradually rise up instead of sinking down, and the gyrations on the pivot would be the contrary way. I had a gyroscope made to test this and found that I was right.

In the spring of 1857 professor Winlock, superintendent of the

American Ephemeris and Nautical Almanac, sent me an invitation, through Dr. B. A. Gould, with whom I had correspondence, to take a part in the computations of that work. I was now connected with a partner in a commercial school in Nashville. I left the school with him and went to Cambridge, Massachusetts, where the office then was, and remained through the summer. I had the choice in the fall of taking a certain amount of job-work yearly, to be done at home in Nashville, or of full employment if I wished to move to Cambridge. I took the latter, but took work with me to do in Nashville while I settled up my affairs there and made arrangements to move. I gave up the school to my partner and returned again to Cambridge in the spring of 1858. I now commenced the preparation of my paper on the "Motions of Fluids and Solids Relative to the Earth's Surface." This was published in parts in *Runkle's Mathematical Monthly*, which was now being started. A few copies were also printed in a connected form and distributed; a full abstract, omitting the mathematics, was published in the January number of Silliman's Journal for 1861.

In this paper the subject contained in my previous popular paper, just referred to, was treated in a mathematical way. In my former paper I had shown that if a body moves on the earth's surface either toward the east or west it is deflected toward the right in the northern hemisphere and the contrary in the southern, just as it is if it moves either toward the north or south, but I had not perceived that these deflecting forces were always equal. This was brought out in the latter paper, and the important result obtained that "*in whatever direction a body moves on the earth's surface there is a force arising from the earth's rotation which deflects it toward the right in the northern hemisphere, but the contrary in the southern.*" This result was published in the monthly in May, 1858, about six months before the subject was fully discussed in the French Academy, in which a number of eminent scientists took part and in which the same general result was arrived at. A reprint of this paper, with notes by Prof. Frank Waldo, has been published by the Signal Service, Professional Paper No. VIII.

In December, 1864, I read a paper before the American Academy of Arts and Sciences, Boston, entitled "Note on the Influence of the Tides in Causing an Apparent Acceleration of the Moon's Mean Motion."

At this time it had been shown by Delaunay and Adams that the

lunar theory did not give the observed acceleration of about $1''$ per century, as had been supposed, but only about $6''$, and there was needed something to account for the balance. The object of the paper was to show that upon a very reasonable and probable lagging of the tides the effect would be sufficient to cause this apparently by changing a little the length of the day, the unit of time. This paper was read only a few weeks before Delaunay read a similar paper before the French Academy.

About this time I discovered two converging series with simple laws expressing the ratio between the circumference and the diameter of a circle. These I showed to Professor Peirce, who requested me to allow him to lay them before the National Academy of Sciences, then about to meet. They were afterwards published by the Smithsonian Institution under the title "New Converging Series expressing the Relation between the Diameter and the Circumference of a Circle," *Contributions of the Smithsonian Institution*, No. 233. The one of these is very convergent, as much so as any now known except one, that of Machin. About this time I commenced my "Tidal Researches," which were originally intended for publication by the Smithsonian Institution. These occupied my spare time from other duties for several years.

In the summer of 1867, soon after Professor Peirce took charge of the Coast Survey, he offered me a partial position in connection with that work, the duty assigned to me being a special discussion of the tidal observations of the Coast Survey. The understanding was that I was still to retain my Nautical Almanac work.

My reports of these discussions appeared in the appendices of the Coast Survey reports. My "Tidal Researches" were completed some time after I was connected with the Coast Survey, and so it was thought best that they should be published by the Coast Survey instead of the Smithsonian Institution, and so they were published as a separate appendix to the Coast Survey Report of 1874 and comprised 270 quarto pages. Only 500 copies were printed and distributed. I now engaged to prepare a series of papers for the Coast and Geodetic Survey, "Meteorological Researches for the Use of the Coast Pilot." These appeared in three parts in the reports of the Survey as they were completed.

Part I. "On the Mechanics and the General Motions of the Atmosphere," 1875.

Part II. "Cyclones, Tornadoes, and Waterspouts," 1878.

Part III. "Barometric Hypsometry and Reduction to Sea Level,"
1881.

In the year 1880 I first became interested in the subject of my "Maxima and Minima Tide-predicting Machine." A paper was read on the subject at the meeting of the American Association for the Advancement of Science, which took place at Boston that year. It was afterwards decided to have the machine constructed by the Coast Survey for its use, and it is now being used in tidal predictions. The report upon the theory and construction of the machine is contained in the report of the Coast and Geodetic Survey for 1883.

In the year 1882 I engaged in the work of the Signal Service Office, reserving, however, a part of my time for the Coast and Geodetic Survey; for the construction of the machine was then only fairly commenced, and it was necessary for me to superintend the work and to report upon the whole matter, and also to apply it in practice and to instruct some one in the use of it. I had several years before this resigned my Nautical Almanac work, which was merely computing and a constant interruption and annoyance in my other researches. My work for the Signal Service is contained in the professional papers and in the reports of the Chief Signal Officer.

Of the leading traits of my character a prominent one all through my life has been a great diffidence and a backwardness in coming in contact with strangers or in putting myself forward in any manner. An example of this occurred in the reading of my first scientific paper before the American Academy at Boston, referred to in the preceding pages, with reference to the effect of "Tidal action in causing an apparent acceleration of the moon's motion." Although the paper contained an original and important suggestion and I had it all written out, yet I carried it to the meetings of the Academy time after time with the intention of reading it, and my courage failed, and if I had deferred it one time more I would not have anticipated Delaunay on the same subject. All the government positions I have had were offered me without solicitation on my part.

Another has been great perseverance in anything in which I get interested, in never giving up anything until I make something out of it or satisfy myself fully that I cannot.

I have always been interested mostly in original researches—any-

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thing in which there was a prospect of discovering something or explaining something which had never been explained.

I have always been inclined to original methods instead of following usual methods which had been followed or laid down before.

In the demonstrations of propositions at college, at recitations I never followed closely the methods given in the text books and often gave a demonstration entirely different.

I have never been a great reader or a hard worker except when I got interested in some subject of research. I then devoured everything I could find to read on the subject and studied on it almost night and day.

Much of my time has been wasted, especially the earlier part of it, because not having scientific books and scientific associations I often had nothing on hand in which I was specially interested.

(Signed)

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PUBLICATIONS OF WILLIAM FERREL.

1. On the effect of the sun and moon upon the rotatory motion of the earth. (Dated Allensville, Kentucky, September 24, 1853.)
Gould's Astronomical Journal, 1853, III, 138-142.
2. On the hypothesis of the internal fluidity of the earth. (No date.)
Nashville Journal of Medicine and Surgery, September, 1854, VII, 199-203.
3. On vision. (No date.)
Nashville Journal of Medicine and Surgery, January, February, and March, 1855, VIII, 22-28, 92-102, 192-201.
[In the article "Binocular Vision," N. J. M. and S., October, 1855, IX, 318-321, the editor, Dr. W. K. Bowling, defends Ferrel's priority over Prof. W. B. Rogers.]
4. On the variable star Algol. (No date.)
Nashville Journal of Medicine and Surgery, April, 1855, VIII, 277-282.
5. The problem of the tides with regard to oscillations of the second kind. (Dated Nashville, May 24, 1856.)
Gould's Astronomical Journal, 1856, IV, 173-176.
6. The rotascope. (Dated Nashville, July 17, 1856) [with an editorial postscript by Prof. W. K. Bowling].
Nashville Journal of Medicine and Surgery, September, 1856, XI, 209-211.
7. An essay on the winds and currents of the ocean. (Dated Nashville, October 4, 1856.)
Nashville Journal of Medicine and Surgery, October and November, 1856, XI, 287-301, 375-389.
Reprinted in Professional Papers of the Signal Service, No. XII, Washington, 1882.
8. A complete solution of the problem of the rotascope, based upon well-known mechanical principles. (Dated Nashville, October 24, 1856.)
Nashville Journal of Medicine and Surgery, December, 1856, XI, 463-473.
9. The influence of the earth's rotation upon the relative motion of bodies near its surface. (Dated Cambridge, November, 1857.)
Gould's Astronomical Journal, 1858, V, 97.

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10. The influence of the earth's rotation upon rotating bodies at its surface. (Dated Nashville, May, 1858).

Gould's *Astronomical Journal*, 1858, V, 113-114.

11. The motions of fluids and solids relative to the earth's surface. [Written in 1858 and 1859, but the concluding section is dated Cambridge, February, 1860.]

The *Mathematical Monthly*, January, 1859,--August, 1860: I, 140-148, 210-216, 300-307, 366-373, 397-406; II, 89-97, 339-346, 374-390.

Separate reprint under the title "The . . . surface; comprising applications to the winds and the currents of the ocean." 4to, 72 pp., New York and London, 1860.

Republished, with notes by Prof. Frank Waldo, as *Professional Papers of the Signal Service*, No. VIII, Washington, 1882.

12. The motions of fluids and solids relative to the earth's surface. (No date.)

[This is a popular presentation by the author of the results deduced in the preceding mathematical memoir No. 11, and is not a simple abstract.]

American Journal of Science, January, 1861 (2), XXXI, 27-61.

Republished in *Professional Papers of the Signal Service*, No. XII.

13. Narrative of the American expedition to Northwest British America to observe the total eclipse of the sun, July 18, 1860. (No date.) (Report to Captain C. H. Davis, United States Navy, Superintendent Nautical Almanac).

American Journal of Science, January, 1861, XXXI, 139-142.

14. Convenient formulæ for interpolation. (No date.)

The *Mathematical Monthly*, September, 1861, III, 377-384.

15. On the cause of the annual inundation of the Nile. (Dated Cambridge, December 12, 1862.)

American Journal of Science, January, 1863 (2), XXXV, 62-64.

16. Note on the influence of the tides in causing an apparent acceleration of the moon's mean motion. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, December 13, 1864, VI, 379-383.

17. Communication supplementary to the preceding. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, February 14, 1865, VI, 390-393.

18. On certain new converging series, expressing the ratio between the circumference and diameter of a circle. (No date.)

Read before the *National Academy of Sciences*, Northampton, August, 1865, Rep. Nat. Acad. Sci. for 1883, p. 36.

Smithsonian Contributions, XVIII, No. 233, printed April, 1871.

19. On certain formulæ of interpolation. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, September 12, 1865, VII, 2-12.

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20. On an annual variation in the daily mean level of the ocean and its cause. (No date.)

Proceedings of the American Academy of Arts and Sciences, Boston, October 10, 1865, VII, 31-36.

21. On a table for facilitating the conversion of longitude and latitude into right ascension and declination. (Read before the National Academy of Sciences, Northampton, August, 1866—title only.) Report Nat. Acad. Sci. for 1883, 37.

22. On tidal researches. (Read before the National Academy of Sciences, Washington, April, 1869—title only.)

Report Nat. Acad. Sci. for 1883, 39.

23. Some results of the discussion of the Boston dry-dock tide observations. Proc. A. A. A. S., Salem, August, 1869, 276 (title only.)

24. Discussion of the tides of Boston harbor. (Dated Cambridge, May 2, 1870.)

U. S. Coast Survey Report for 1868, pp. 6, 7 and Appendix No. 5, 51-102.

25. Explanation of the observed fact that a man's left side walks faster than his right (title only). (Read before the National Academy of Sciences, Washington, April, 1871.)

Rep. Nat. Acad. Sci. for 1883, p. 41.

26. Determination of the moon's mass from tidal observations. (Title only). Read before the National Academy of Sciences, Washington, April, 1871.

Rep. Nat. Acad. Sci. for 1883, p. 41.

27. On the moon's mass as deduced from a discussion of the tides of Boston harbor. (Dated Cambridge, June 24, 1871.)

U. S. Coast Survey Report for 1870, Appendix No. 20, 190-199.

28. The cause of low barometer in the polar regions and in the central part of cyclones. (Dated Cambridge,)

Nature, July 20, 1871, IV, 226-228.

Van Nostrand's Engineering Magazine, November, 1871, V, 525-527.

Professional Papers of the Signal Service, No. XII, Washington, 1882.

29. On the effects of winds and barometric pressure on the tides of Boston and on the mean level of the sea.

Philosophical Society of Washington, Bulletin, May 4, 1872, I, 53-54.

30. Maxima and minima of tides on the coast of New England for 1873. (Dated Cambridge, September 2, 1872.)

U. S. Coast Survey Report for 1872, Appendix No. 7, 73, 74.

31. Tidal researches. Read before the National Academy of Sciences, Cambridge, November, 1872 (title only).

Rep Nat. Acad. Sci. for 1883, p. 42.

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32. Meteorological effects upon the heights of the tides. (Dated Cambridge, January 2, 1873.)

American Journal of Science, May, 1873 (3), V, 342-347.

33. Report on meteorological effects upon tides from observations. (Dated Cambridge, May 31, 1873.)

U. S. Coast Survey Report for 1871, Appendix No. 6, 93-99.

34. Table of tide predictions for Boston harbor for 1874. [Contributed by William Ferrel to the annual volume of "Tide Tables for the United States."]

See U. S. Coast Survey Report for 1873, 62.

35. On the tides of Tahiti. Read before the National Academy of Sciences, Washington, April, 1874 (title only).

Report Nat. Acad. Sci. for 1883, p. 44.

36. On the laws of cyclones. Read before the National Academy of Sciences, Washington, April, 1874 (title only).

Report Nat. Acad. Sci. for 1883, p. 44.

37. Tidal researches. (Dated Washington, May, 1874.)

[Referred to in U. S. Coast Survey Report for 1874, page 12, as a separately printed Appendix.] Washington and Cambridge, 1874, 4to, 270 pages.

38. On the law connecting the velocity and direction of the wind with the barometric gradient. (No date.)

Abstract in Bull. Phil. Soc., Washington, June 20, 1874, I, 106-109.

39. Relation between the barometric gradient and the velocity of the wind. (No date.) Read before the American Association for the Advancement of Science, at Hartford, August, 1874.

Proc. A. A. A. S., Hartford, August, 1874, p. 144 (title only).

American Journal of Science, November, 1874, VIII, 343-362.

See Hann's presentation of the result in Zeit. Oest. Gesell. Met., 1875, X, 81-88, 97-106.

40. The constant currents in the air and the sea. (Dated Washington, November 7, 1874.)

Nature, January 7, 1875, XI, 186-187.

41. Beziehung zwischen den barometrischen gradienten und die windgeschwindigkeit (letter dated Washington, April 27, 1875).

Zeitschrift Oest. Gesell. fur Meteorologie, August 15, 1875, X, 254-255.

Translation in Ann. Rep. Sec. Smithsonian Inst. for 1877, 445-446.

42. Discussion of the tides in New York harbor. (Dated Washington, June 30, 1875.)

U. S. Coast Survey Report for 1875, page 8, and Appendix No. 12, 194-220.

43. Tides of New York harbor. (Title only.)

Proceedings A. A. A. S., Detroit, August, 1875, p. 121.

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44. On a controverted point in Laplace's theory of the tides. (Dated November 17, 1875.)

L., E. & D. Philosophical Magazine, March, 1876 (5), I, 182-187.

45. On the progressive motion of storms. (Title only.) Read before the National Academy of Sciences, Washington, April, 1877.

Report Nat. Acad. Sci. for 1883, p. 47.

46. Methods, discussions, and results.

Meteorological Researches for the use of Coast Pilot: part I, Washington, 1877. On the mechanics and the general motions of the atmosphere. (Preface by Superintendent C. P. Patterson, dated July 21, 1877.)

Also published in U. S. Coast Survey Rep., 1875, as Append. 20, 369-412, Washington, 1878.

Review by M. Thiessen, Zeit. Oest. Gesell. Met., 1879, XIV, 386-390.

Notice of Thiessen's review, Coast Survey Rep., 1879, 4, Washington, 1882.

47. The theory of waterspouts. (Title only.) Read before the National Academy of Sciences, Washington, April, 1878.

Report Nat. Acad. Sci. for 1883, p. 48.

48. Observation of total solar eclipse, July 29, 1878, on the summit of Gray's peak, Colorado.

(Noticed in U. S. Coast and Geodetic Survey Report for 1879, 65.)

[Probably an unpublished manuscript report.]

49. Discussion of tides in Penobscot bay. [At Pulpit cove.] (Dated Washington, December 31, 1878.)

U. S. Coast and Geodetic Survey Report for 1878, Appendix 11, 268-304, Washington, 1881.

[See reference to progress on this memoir in Report C. and G. Survey, 1880, p. 2.]

50. Tornadoes, waterspouts, and hailstorms. (Title only.)

Proc. A. A. A. S., St. Louis, August, 1878, 123.

51. On hollow waterspouts and sandspouts. (Title only.) Read before the National Academy of Sciences, Washington, April, 1880.

Report Nat. Acad. Sci. for 1883, p. 51.

52. On cloud-bursts. (Title only.) Read before the National Academy of Sciences, Washington, April, 1880.

Report Nat. Acad. Sci. for 1883, p. 52.

53. Methods and results.

Meteorological Results for the use of the Coast Pilot. Part II: On cyclones, waterspouts, and tornadoes. [No date, but contains data added in 1880. See notice of its completion in Rep. C. S. O., 1881, p. 2.] Washington, 1880.

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Also published as U. S. Coast Survey Report for 1878, Appendix No. 10, 176-267, Washington, 1881. [This volume was bound and distributed in July, 1882.]

Review of the above by (. . . ?) in *The Scientific American*, 1881, XLIV, 304; by E. D. Archibald in "*Nature*," 1882, XX, 155, 291; also XXVI, 9, 31; by A. Sprung, *Zeits. Oest. Gesell. Met.*, 1882, XVII, 161-175, 276-282.

54. Maxima and minima tide-predicting machine. [Sketch of a proposed construction.] (Title only.)

Proc. A. A. A. S., August, 1880, Boston, p. 276.

55. Meteorological researches. Part II: Cyclones, tornadoes, and water-spouts. [Abstract of memoir No. 53, by the author.] (No date.)

American Journal of Science, July, 1881 (3), XXII, 33-48.

Professional Papers of the Signal Service, No. XII, Washington, 1882.

Review of this abstract in "*Naturforscher*," 1881, XIV, 345-348.

56. Methods and results.

Meteorological Researches for the use of the Coast Pilot. Part III: Barometric hypsometry and reduction to sea-level. (No date, evidently 1881.) Washington, 1882.

Also published as U. S. Coast and Geodetic Survey Report for 1881, Append. No. 10, 225-269, Washington, 1883.

See report of progress on this work, C. and G. Survey Rep. for 1880, p. 2, and for 1881, p. 2.

Review by Hartl, *Zeits. Oest. Gesell. Met.*, 1884, XIX, 466-468.

57. On the conditions determining temperature. (No date.)

Bull. Phil. Soc., Washington, January 28 and February 25, 1882, V, 90-97.

Review in *Zeits. Oest. Gesell. Met.*, 1884, XIX, 386-388, 500-501.

58. Discussion of the tides of the Pacific coast of the United States. (Dated Washington, June 1, 1882.)

U. S. Coast and Geodetic Survey Report for 1882, Append. No. 17, 437-450, Washington, 1883.

59. Wind pressure. (Dated Washington, June 20, 1882.)

Van Nostrand's Engineering Magazine, August, 1882, XXVII, 140-143.

60. The relative temperatures of the two hemispheres of the earth. (No date.)

Am. Jour. Sci., August, 1882 (3), XXIV, 89-92.

61. Maxima and minima tide-predicting machine. Exhibition of the machine. (Title only.) Read before the National Academy of Sciences, Washington, April, 1883.

Rep. Nat. Acad. Sci. for 1883, p. 56.

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62. Temperature of the atmosphere and the earth's surface. (Dated Washington, June 30, 1883.)
Professional Papers of the Signal Service, No. XIII, 4to, 69 pp., Washington, 1884.
Review in Zeits. Oest. Gesell. Met., 1884, XIX, 500-501.
63. Report on the harmonic analysis of the tides at Sandy Hook. (Dated Washington, July 31, 1883.)
U. S. Coast and Geodetic Survey Report, 1883, Append. No. 9, 247-252, Washington, 1884.
64. Description of a maxima and minima tide-predicting machine. (Dated Washington, November 10, 1883.)
U. S. Coast and Geodetic Survey Report for 1883, Append. No. 10, 253-273.
[See references to the designing and constructing, Rep. C. and G. Survey, 1880, p. 3; 1882, p. 61; and to its satisfactory completion, Rep. C. and G. Survey, 1883, p. 93.]
65. Maxima and minima tide-predicting machine. (No date.) [Abstract by the author of the preceding memoir.]
Science, April 14, 1884, III, 408-410.
66. On the harmonic analysis of the tides at Governor's island, New York harbor. (Dated Washington, June 30, 1884.)
U. S. Coast and Geodetic Survey Report for 1885, Append. 13, 489-493. [See also remarks by the Superintendent C. and G. Survey on page 7.]
67. The gyration of a vibrating pendulum. (No date.) [Letter correcting a typographical error in "Motion of fluids and solids."]
Science, July 18, 1884, IV, 53.
68. Priorität des Buys-Ballot'schen Gesetzes. (Letter dated Washington, March 25, 1885.)
Zeits. Oest. Gesell. Met., 1885, XX, 187.
69. Recent advances in meteorology. [Original dated March 2, 1885; revised in reading proof-sheets, July to September, 1886.]
Report of Chief Signal Officer, 1885, part II, Appendix No. 71, 440 pp.
Review by W. M. Davis, Science, June 3, 1887, IX, 539-541 translated in Met. Zeit., 1887, IV, p. [57]-[59].
Review by G. E. Curtis, Am. Met. Jour., August, 1887.
70. Psychrometry. [Abstract of preliminary results, see No. 73.]
Proc. A. A. A. S., Ann Arbor, August, 1882, p. 80.
71. Vapor tension, dew-point, and relative humidity tables, adapted to the whirled or sling-psychrometer.
Published by the Signal Office as official: signed by Thomas M. Woodruff, November 27, 1885. 12 pp., 8vo.
[These were officially adopted by general orders No. 1, January 1, 1886, and agree with the tables appended to No. 73 of this bibliography.]

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72. The solar thermometer. (No date.)
American Meteorological Journal, November, 1885, II, 303-306.
73. Report on psychrometric tables for use in the Signal Service. (Dated December 10, 1885.)
Report of Chief Signal Officer, 1886, Append. 24, 233-259.
74. The Arago-Davy actinometer. (Dated Washington,)
American Meteorological Journal, December, 1885, II, 350-354, 395-400.
75. Temperature of the moon. (First letter, no date.)
Science, December 18, 1885, VI, 541-542.
76. Temperature of the moon. (Second letter, Washington, January 4, 1886.)
Science, January 8, 1886, VII, 32.
77. Sea-level and ocean currents. (First letter, dated Washington, January 18, 1886.)
Science, January 22, VII, 75-77.
78. Temperature of the moon. (Third letter, dated Washington, January 28, 1886.)
Science, February 5, 1886, VII, 122-123.
79. A course of forty lectures to signal officers on "Dynamic meteorology" during February and March, 1886.
[Not printed as yet, but made the basis of the subsequent "Popular treatise on the winds."]
80. Sea-level and ocean currents. (Second letter, dated Washington, February 18, 1886.)
Science, February 26, 1886, VII, 187-189.
81. Review of Rudolph Spitaler's "Wärme-vertheilung, &c." (no date.)
Am. Met. Jour., March, 1886, II, 520-523.
82. Review of Dr. A. Sprung's "Lehrbuch der Meteorologie" (no date.)
Am. Met. Jour., April, 1886, II, 553-565.
83. Nocturnal cooling of bodies. (No date.)
Science, April 2, 1886, VII, 329-330.
84. Sea-level and ocean currents. (Third letter, dated Washington, July 18, 1886.)
Science, July 30, 1886, VIII, 99-101.
85. Tables for reduction of the barometer to sea-level. [Ordered to be adopted July 10, 1886.]
Published by the Signal Service, July 27, 1886, and Rep. C. S. O., 1886, 226-232.
86. Report on reduction of barometric pressure to sea-level and standard gravity. (Report dated August 31, 1886.)
Rep. of C. S. O., 1886, Append. 23, pp. 221-232.

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87. Review of R. T. Smith's "Results of solar radiation observations." (Dated Washington, September 4, 1886.)
Am. Met. Jour., September, 1886, III, 223-226.
88. Sensitiveness of the wind-vane (dated Kansas City, Mo., January 12, 1887.)
Am. Met. Jour., February, 1887, III, 452-454.
89. Relation of the pressure to the velocity of the wind. (Dated Kansas City, Mo., June 5, 1887.)
Am. Met. Jour., August, 1887, IV, 173-177.
90. Theoretical meteorology. (Letter dated Kansas City, Mo., July 13, 1887.)
Science, July 22, 1887, X, 48.
91. Reply to critical review by G. E. Curtis of "Recent advances in meteorology". (Dated Kansas City, Mo., August 14, 1887.)
Am. Met. Jour., September, 1887, IV, 211-213.
92. Note on the influence of forests upon rainfall. (No date.)
Am. Met. Jour., February, 1889, V, 433-435.
93. Note on the wind-pressure constant. (Letter dated Kansas City, Mo., February 20, 1889.)
Science, March 1, 1889, XIII, 171.
94. The wind-pressure constant. (Letter dated Kansas City, Mo., March 5, 1889, correcting a typographical error.)
Science, March 15, 1889, XIII, 205.
95. Note on the Robinson anemometer constant. (Letter, no date.)
Science, March 15, 1889, XIII, 204-205.
96. A popular treatise on the winds, comprising the general motions of the atmosphere, monsoons, cyclones, tornadoes, waterspouts, hailstorms, &c. (dated Kansas City, Mo., April, 1889.)
John Wiley & Sons, New York, 1889, pp. vii, 505.
Reviewed by W. M. Davis, Science, February 28, 1890, XV, 142-143.
97. Laplace's solution of the tidal equations. (Dated Washington, May 22, 1889.)
Gould's Astronomical Jour., July, 1889, IX, 41-44.
98. The law of thermal radiation. (No date.)
American Journal of Science, July, 1889 (3), XXXVIII, 3-29.
99. Decrease of temperature with increase of altitude. (No date.)
Am. Met. Jour., August, 1889, VI, 145-150.
100. Comments on Mr. Arthur Searle's "Atmospheric economy of solar radiation." (No date.)
Am. Met. Jour., August, 1889, VI, 177-179.
101. Weber's law of thermal radiation (no date).
Am. Jour. Sci., February, 1890 (3), XXXIX, 137-145.

WILLIAM FERREL.

102. Professors Hazen's and Espy's experiments. (Letter dated Martinsburg, West Virginia, September 24, 1890.)

Science, October 3, 1890, XVI, 192-193.

103. Espy's experiments on storm generation. (Dated Martinsburg, West Virginia, September 25, 1890.)

Am. Met. Jour., November, 1890, VII, 337-340.

104. Laplace's solution of the tidal equations. (Dated Martinsburg, West Virginia, December 3, 1890.)

Gould's Astronomical Journal, December, 1890, X, 121-125.

105. Dr. Hann's studies on cyclones and anticyclones. (Dated Martinsburg, West Virginia, December 12, 1890.)

Science, December 19, 1890, XVI, 344-347.

106. The subtropical zones of high barometric pressure. (Dated Martinsburg, West Virginia, December 22, 1890.) [Reply to Hann and others.]

Science, January 2, 1891, XVII, 8-10.

107. Cyclones and areas of high pressure (dated Martinsburg, West Virginia, January 10, 1891).

Science, January 16, 1891, XVII, 38-40.

[A reply to W. M. Davis in Science, May 30, 1890, XV, p. 332, and Science, January 2, 1891, XVII, p. 4; and to H. C. Clayton in Science, January 2, 1891, XVII, pp. 10 and 11.]

108. The high-pressure area of November, 1889, in central Europe, with remarks on high-pressure areas in general. [A reply to Dr. J. Hann.] (Dated Martinsburg, West Virginia, probably March, 1891.)

Nature, April, 1891, XLIII, 466-471.

109. Autobiographical sketch. (Written in January, 1888, for Mr. A. McAdie.)

[Published herewith as an appendix to this biographical memoir.]

MEMOIR
OF
MONTGOMERY C. MEIGS.
1816-1892.

BY
HENRY L. ABBOT.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1893.

BIOGRAPHICAL MEMOIR OF MONTGOMERY CUNNINGHAM MEIGS.

Mr. President and Gentlemen of the Academy:

In the death of General Montgomery C. Meigs the Academy has lost one of its earliest members, who for many years has held an honored place in its Council. When the act of incorporation was passed by Congress in 1865 he was an intimate personal and professional friend of Henry, Bache, Totten, Baird, Saxton, and other leading scientists, and although his busy life afforded scant leisure for individual research his duties and tastes made him an extensive reader in many branches and gave him a warm interest in aiding the objects of the institution. He was a Regent of the Smithsonian Institution and a regular attendant upon the sessions of the Academy, where his familiar presence will long be missed by his associates.

General Meigs was of Puritan ancestry, his family tracing their descent from Vincent Meigs, or Meggs, who, with his sons, John and Mark, settled at New Haven about 1644, after having sojourned for a time at Weymouth, Massachusetts. They seem to have come from the southern part of England, probably from Dorsetshire. The descendants of Vincent Meigs for four generations lived and died in Connecticut.

The representative of the family in this fourth generation, Return Meigs, was about 67 years old when the first blood of the Revolutionary war was shed at Lexington, and was therefore too old to take an active part in the struggle. His son, Return Jonathan Meigs, born in 1740, won distinction as a soldier. He served as major in the expedition against Quebec, where he was captured; commanded a regiment under General Anthony Wayne at the storming of Stony Point, and still later led a successful expedition against Sag Harbor, on Long Island, for which Congress voted him a sword that is still preserved in the family. After peace was declared he wandered to Ohio with many other impoverished officers

of the army, and thence to Tennessee, where he became commissioner to the Cherokee Indians. His son, of the same name, remained in Ohio and represented that State in the United States Senate in 1808-'10; was governor in 1810-'14, and subsequently held the office of Postmaster General during the administrations of Madison and Monroe.

Josiah Meigs, a brother of Colonel Return Jonathan Meigs and 17 years his junior, was the grandfather of our colleague. He was a graduate of Yale College, where he became a tutor and subsequently professor of mathematics and natural philosophy. His life was a checkered one, as he had little sympathy with the Federal party; and he removed, in consequence, first to Bermuda and thence to Georgia, where he became professor and soon thereafter president of the struggling university of that State. In 1812 he was appointed Surveyor General at Cincinnati, and in 1814 Commissioner of the General Land Office at Washington. Here he remained until his death, in 1822. In 1817 he attempted to obtain legislation by Congress authorizing the President to cause meteorological registers to be kept at the land offices under his direction, thus anticipating the Smithsonian and army records and the present Weather Bureau. Meteorology was a subject to which he had given life-long attention, and failing to obtain the desired official action, he inaugurated in 1819 a voluntary system of observations at the land office stations which was continued several years and the records of which are still preserved. Josiah Meigs was a member of the American Philosophical Society and of the American Academy of Languages and Belles-Lettres, and was a man distinguished for learning and independence of character.

Charles Delucena Meigs, the father of our associate, was a distinguished physician of Philadelphia, and for twenty years a professor in the Jefferson Medical College. His wife, Miss Mary Montgomery, of New Jersey, was of the same Scotch family from which was descended General Richard Montgomery, who commanded the American forces and fell gallantly before Quebec on December 31, 1775. Her motherly influence upon her children is apparent from a letter written by General Meigs at the age of 72 years, after she had been in her grave quite 23 years. He wrote: "She lived for her husband and children, not for herself, and all her pleasures were in their happiness and success. The longer I live the more strongly her worth and devotion are impressed upon

me. I am more able as I grow older to understand and appreciate her virtues."

After completing his education at the University of Pennsylvania, Dr. Charles Delucena Meigs began the practice of his profession at Augusta, Georgia, where his eldest son, the subject of this memoir, was born on May 3, 1816. His parents soon removed to Philadelphia, where the boy was educated, finally entering the University of Pennsylvania at the early age of fifteen, but leaving it before graduation, upon receiving an appointment at the Military Academy at West Point. Here he reported in the summer of 1832, and was graduated on July 1, 1836, ranking fifth in a class of forty-nine members. His diploma recommended him for any branch of the service.

After a short service as second lieutenant in the First Artillery he was transferred to the Corps of Engineers, in which he reached the grade of first lieutenant in 1838, and that of captain in 1853. Shortly after the outbreak of the civil war he was appointed Quartermaster General with the rank of brigadier general, and he served in this capacity until retired for age, on February 6, 1882. He received the brevet of major general for "distinguished and meritorious services during the rebellion," to date from July 5, 1864.

His claims to distinction rest upon important professional services as an engineer, and upon able administration as chief of the most responsible supply department of the army during the trying period of the civil war. His record in both branches of the service is brilliant.

As an engineer, he was engaged on several minor works of coast defense; devised and constructed the Washington aqueduct; constructed the extension of the National Capitol, with its new wings and iron dome; extended the General Post Office building at Washington, D. C.; prepared in conjunction with Professor Baird the plans for the new National Museum to preserve and exhibit the contributions from the Centennial Exhibition of 1876; and, after his retirement, acted as architect of the Pension Office building during the entire period of its erection. These works afforded opportunities for the exhibition of boldness and originality in construction, and they will remain the best monuments to his ability as an engineer.

Among these structures the Washington aqueduct was always his favorite. To make the surveys, plans, and estimates of this great work was the first conspicuous and responsible duty which devolved

upon him in his professional career. He was then thirty-six years of age, and he entered upon the task with the enthusiasm of youth and the energy of matured manhood. The preliminary studies were made with wonderful rapidity, but so thoroughly as to leave little room for improvement during construction.

The aqueduct proper is a circular conduit of brick or rubble masonry, laid in hydraulic cement. It is nine feet in interior diameter and eleven miles long, measured from the Great Falls of the Potomac to the distributing reservoir in Georgetown. The general slope is nine and a half inches per mile. Whenever possible the location is subterranean, generally near the surface, in the natural soil, but several tunnels and supporting bridges could not be avoided, as the route traverses the lines of natural drainage of the District. The capacity is seventy millions of gallons per day.

This is not the place to detail the interesting devices for purifying the supply and regulating the flow at the receiving and distributing reservoirs, or the system of distribution throughout the cities by iron pipes, but mention should be made of two of the supporting bridges which illustrate the boldness and originality of the mind of the engineer. One is on the line of the masonry conduit at the crossing of Cabin John branch, and the other where the main supply pipes cross Rock creek, which separates Georgetown from Washington city.

The Cabin John bridge is a single masonry arch 220 feet in span, 101 feet in height, and 20 feet wide. The original project contemplated a series of short arches upon the Roman plan, but the change was made in view of the monumental character of the work, which was considered to justify the increased expense. The construction is notable as being the longest stone arch in existence; it is justly admired for its boldness and beauty.

The Rock Creek bridge is a novel structure, which fulfills the double purpose of conveying the main supply pipes across the stream and at the same time of serving as a viaduct. It is composed of two iron pipes each forty-eight inches in diameter, which carry the water supply and serve as the ribs of an arch of 200 feet span and 20 feet rise; this and a bridge similar, but of shorter span, crossing College branch, are unique structures, which testify to the originality characteristic of the designs of General Meigs.

The date of his assignment to the charge of the Washington aqueduct was November 3, 1852. In the following March the

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charge of the Capitol extension was added, and in April, 1855, that of the extension of the General Post Office. The administration of these works included not only the supervision of numberless engineering details, great and small, but also the disbursement of the large appropriations for which, by the regulations of the army, the officer in charge is held responsible. The aggregate of Captain Meigs' engineer disbursements exceeded ten million dollars. It was at this period that my acquaintance with him began. Washington city was then a much smaller place than now, and popular interest in the prosecution of these great public works drew general attention to the management of the officer in charge. I well remember hearing from total strangers expressions of admiration and surprise that any one could successfully perform so much and so diversified labor. This well earned reputation for executive ability no doubt contributed to bring about his rapid promotion in the army when the political difficulties of the period culminated in the civil war.

The attention of influential men at this period was still further drawn to Captain Meigs by the fact that in the autumn of 1860 he incurred the ill will of the Secretary of War, John B. Floyd, for his independent stand regarding certain large contracts, and he was suddenly relieved from duty in Washington and banished to Tortugas, in the Gulf of Mexico, to construct fortifications at that place and at Key West. The resignation of Secretary Floyd in January, 1861, led to his recall to Washington in February, and his reassignment to the charge of the construction of the aqueduct; but the rising war cloud was then absorbing the attention of every one, and Captain Meigs was no exception to the rule.

His first opportunity for active participation in the great events then in progress came in connection with the relief of Fort Pickens. The circumstances were so unusual and so characteristic of the times that they will be briefly noted. He wrote himself under date of March 15, 1865:

"This earliest expedition of the war was organized under exceptional circumstances, and its records do not appear to have been preserved in Washington.

"Inquiry at the Navy Department and at the Executive Mansion and at the State Department has failed to discover any copies of the order.

"It was an executive act, unknown at the time to any but those

engaged therein, including General Scott, the Secretary of State, and the President."

The ordinance of secession was passed by the Florida convention on January 10, 1861. Two days thereafter the navy yard at Pensacola and Forts Barrancas and McRae were seized; and Lieutenant Adam J. Slemmer, First Artillery, then commanding the land forces, evacuated Barrancas barracks and concentrated his command (his own Company G, of 46 men and 30 sailors) at Fort Pickens, on Santa Rosa island, the most defensible position in the harbor. In the same month Captain Israel Vogdes, First Artillery, with his company of 86 men, was transferred from Fort Monroe to the steamer Brooklyn to reinforce him, but before their arrival at Fort Pickens an armistice or agreement was entered into between the Government and the State authorities, by virtue of which the troops were kept afloat off Santa Rosa island. This was the condition of affairs when President Lincoln was inaugurated on March 4. On March 12 an order was sent by General Scott directing a landing and the reinforcement of Fort Pickens. This order was not received until April 1, and was then communicated at once to Captain H. A. Adams, commanding the naval forces off Pensacola; but as the latter felt himself bound by the agreement above noted he hesitated to act, and sent Lieutenant Gwathmey, U. S. N., by land to report the fact to the new Secretary of the Navy, Gideon Welles, and ask instructions. This officer arrived in Washington on April 6, and Lieutenant John L. Worden, of Monitor fame, was on the same night dispatched over land with orders to Captain Adams to obey the instructions of General Scott and disembark the troops, which it had been supposed was accomplished long before. These orders came to hand on April 12, and on that night Captain Vogdes, with his company of 86 men and 115 marines, was successfully landed, increasing the garrison to nearly 300 men. The bombardment of Fort Sumter was then in progress, and it has been claimed that but for this reinforcement General Bragg would have attacked Fort Pickens on the night of April 13. The official records do not support this assertion. The field return of General Bragg's troops on March 31 shows an aggregate present of only 1,116 men, all Infantry. On April 6 he reported to the Confederate War Department:

"The only attack which I could hope to make now would be a sudden dash, distracting the enemy by a false attack and scaling

the walls in an opposite direction. The weakness of the garrison and the ardor and ignorance of my troops would be strong elements of success."

On April 8 he received a reinforcement of 1,600 men; on April 12 he reported:

"Alarm guns have just fired at Fort Pickens. I fear the news" (attack on Sumter?) "is received, and it will be reinforced before morning. It cannot be prevented."

Late in March, Secretary Seward, becoming solicitous for the safety of Fort Pickens, or perhaps for other reasons more nearly concerning the Department of State, decided to induce the President to dispatch another relief expedition, without consultation either with the Secretary of War or the Secretary of the Navy. He had confidence in the ability of Captain Meigs and Lieutenant D. D. Porter, U. S. N., to arrange all the technical details, and accordingly went with them to the President. The following extracts are from a narrative written by General Meigs under date of September 14, 1865:

"My first interview with the President and the Secretary of State in relation to the matter was on the evening of the 29th of March. The President did not inform me that he intended to attempt to relieve Fort Sumter, but questioned me as to the possibility of doing it. I advised him in general terms that I could find him plenty of officers of the navy willing to try it. He then asked me whether I thought Fort Pickens could be reinforced. I replied that it could be, provided the relieving force reached there before it fell, and with the map before us the mode of effecting this object was discussed. I advised that if the attempt was made a fleet steamer, under a young and enterprising officer, should be dispatched immediately to run the batteries, enter the harbor, and prevent any expedition of Bragg's crossing the harbor in boats to assault Fort Pickens. The President said he would see me again if he concluded to go further in the matter.

* * * * *

"I myself suggested to the President the name of the Powhatan and of her commander" (Lieutenant Porter), "and prepared the orders in relation to the movement for his signature, and this I did on the 31st of March or the 1st of April, three or four days before the Fort Sumter expedition was resolved upon.

* * * * *

"This the first successful military expedition of the war originated with Mr. Seward. Until it sailed the United States had declined everywhere; fortresses and harbors had been lost. He carried me to the President, merely saying that he thought the President ought to see some of the younger officers and not consult only with men who, if war broke out, could not mount a horse."

In his *Fifty Years' Observation of Men and Events*, General Keyes further contributes to this interesting history. He states that he was sent by General Scott, whose military secretary he then was, on Easter Sunday, March 31, to Secretary Seward to explain the difficulty of relieving Fort Pickens. The Secretary summoned Captain Meigs and ordered them jointly to prepare a plan for its relief, which they did on the same day, and, introduced by Secretary Seward, laid it before the President. The latter ordered them to see General Scott and carry out the plan. They drafted the order to Colonel Brown, placing him in command of the expedition, and General Scott signed it on April 1. Another order, giving carte blanche to Colonel Keyes to fit out the army contingent, General Scott declined to sign, referring him to the President, who signed it on April 3. Keyes, Meigs, and Porter left Washington together that same night and began work in New York on the following day. This incident ultimately brought about the dismissal of Colonel Keyes from his position as General Scott's military secretary.

But on April 1 the Powhatan had been already selected by the Secretary of the Navy as the flagship of the expedition to relieve Fort Sumter, and was put under orders for sea service; nevertheless, armed with the following document, drafted by Captain Meigs, Lieutenant Porter obtained possession of the ship from the officer in command, Captain Samuel Mercer, U. S. N.:

"EXECUTIVE MANSION,

"WASHINGTON, *April 1, 1861.*

"Lieut. D. D. Porter will take command of the steamer Powhatan, or any other U. S. steamer ready for sea which he may deem most fit for the service to which he has been assigned by confidential instructions of this date.

"All officers are commanded to afford him all such facilities as he may deem necessary for getting to sea as soon as possible.

"He will select the officers to accompany him.

"Recommended: William H. Seward.

"ABRAHAM LINCOLN."

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This interference with his cherished plans came to the knowledge of the Secretary of the Navy late at night on April 5, and at a midnight interview with the President, at which Mr. Seward also was present, he obtained a revocation by telegraph of this order detaching the flagship of the Fort Sumter expedition. The telegram, however, arrived too late to stop Lieutenant Porter, who, as he states, deliberately disregarded it and sailed on April 6, arriving at Santa Rosa Island on April 17.

Captain Meigs personally sailed on the transport *Atlantic* as the engineer officer of the expedition, with orders to return as soon as it was established at Fort Pickens. The force consisted of four companies under command of Brevet Colonel Harvey Brown. Shortly before sailing, on April 7, Captain Meigs wrote to Secretary Seward as follows :

"Unless this movement is supported by ample supplies and followed up by the navy, it will be a failure. This is the beginning of the war which every statesman and soldier has foreseen since the passage of the South Carolina ordinance of secession. You will find the army and the navy clogged at the head with men, excellent patriotic men, men who were soldiers and sailors forty years ago, but who now merely keep active men out of the places in which they could serve the country.

"If you call out volunteers you have no general to command. The general born, not made, is yet to be found who is to govern the great army which is to save the country, if saved it can be. Colonel Keyes has shown intelligence, zeal, activity, and I look for a high future for him.

"England took six months to get a soldier to the Crimea. We were from May to September in getting General Taylor before Monterey. Let us be supported; we go to serve our country, and our country should not neglect us or leave us to be strangled in tape, however red."

The *Atlantic* arrived safely at Fort Pickens on April 16, some hours before the Powhatan, and the troops were at once landed on the beach without interference from the enemy. The Powhatan did not enter the harbor, as had been ordered, because this would have entailed a great and needless exposure of the ship. Thus for the second time was Fort Pickens reinforced.

Shortly after his return to Washington Captain Meigs was

appointed Colonel of the Eleventh Infantry, to date from May 14, and Quartermaster General with the rank of brigadier general, to date from May 15, 1861.

This appointment carried with it weighty executive responsibility, as upon the Quartermaster Department is devolved the duty of providing the transportation of every kind needed in the movement of troops and materials of war, of supplying the clothing and camp and garrison equipage of the men, and, in general, of attending to numberless details upon which the military efficiency of the army depends. The department had been carefully organized and trained by previous experience and was officered by able and efficient men, but how to expand sufficiently to meet the demands of the rapidly increasing volunteer service was a problem calling for business training and ability of a high order. These were the qualifications which General Meigs brought to the task and which he displayed throughout the long and weary years of the war in a manner to win the approval of the army and of the country. The usual duties of the department were largely increased by the act approved July 4, 1864, which devolved upon the Quartermaster General the duty of examining and reporting upon all claims of loyal citizens in States not in rebellion for quartermaster stores received or taken for the use of the army during the entire war. Over 33,000 claims, aggregating forty million dollars, were thus reported upon, and about 11,000 of them were paid by Congress upon the recommendation of General Meigs.

He enjoyed throughout the war the confidence and support of the President to a marked degree, and although the nature of his duties kept him from the field, Secretary Seward fully appreciated their importance, as the following extract from a letter written in 1867, introducing him to officers of the diplomatic service abroad, will testify:

"The prevailing opinion of this country sustains a firm conviction which I entertain and on all occasions cheerfully express, that without the services of this eminent soldier the National cause must either have been lost or deeply imperiled in the late civil war."

After the close of the war General Meigs attended to the routine administration of his department, and in addition made several extensive inspections and served on many important boards. He

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twice visited Europe—the first time in 1867-'68, for the benefit of his health, which had suffered from overwork, and the second time in 1875-'76, on special service, to study the constitution and government of armies abroad. He was retired by action of the President on February 6, 1882. The following address to the officers of his department, bearing the same date, so well expresses his views upon the services rendered by that branch of the service during his administration that the language is quoted entire :

“ On this day, having passed the age at which an officer may be retired at the discretion of the Executive, I am relieved by executive order and retired from the military service into which I entered as a cadet 49 years and 7 months since.

“ Of this time nearly 21 years have been spent at the head of your department, to which I had the honor to be called by President Lincoln in 1861.

“ The corps has seen great changes since I entered it. It has been expanded till, leavened by the knowledge and spirit and integrity of the small body of officers who composed it early in 1861, it showed itself competent to take care of the supplies and transportation of a great army during four years of most active warfare. It moved vast bodies of soldiers over long routes ; it collected a fleet of over 1,000 sail of transport vessels upon the great rivers and upon the coast ; it constructed and equipped a squadron of river iron-clads, which bore an important part in the operations of the army in the West, and after having proved its practical power and usefulness, was accepted by the navy, to which such vessels properly belonged ; it supplied the army while organizing and while actively campaigning over long routes of communication by wagon, by rail, by river, and by sea, exposed to hostile attacks and frequently broken up by the enemy ; and, having brought to the camps a great army, it, at the close of hostilities, returned to their homes over a million and a quarter of men.

“ It is now reduced to the proportions of a peace establishment, containing only 64 officers of the staff and about 200 acting assistant quartermasters who hold their commissions in the line.

“ During this time the corps has applied to the wants of the army over nineteen hundred and fifty-six millions six hundred and sixteen thousand dollars and has used this vast sum, nearly two

thousand millions, with less loss and waste from accident and from fraud than has ever before attended the expenditure of such a treasure.

"Its worth and its success have been the study and admiration of military nations. On only two occasions during the four years of war did any army of the Republic suffer from want of supplies. General Rosecrans' army, after the check at Chickamauga, lost control of its long line of communications, men lived for a time on scant rations, and many horses and mules of the cavalry, the artillery, and the trains perished. On taking Savannah, upon his march to the sea, General Sherman found it impossible at once to open the river, whose channels had been during four years laboriously obliterated by the enemy. Thus the quartermaster's fleet, laden with all military supplies, which waited at the mouth of the river the opening of navigation to satisfy all the wants of his army, was detained for a few days, and some animals perished in the Southern savannahs; but vessels and machines provided by the quartermaster department opened the channel and soon restored plenty. I am happy that I was able on these two only occasions of want to be with the troops.

"Believing that should another great war arise you will be able again to quickly enthuse into the officers who must be called from civil life to reinforce you the spirit, the integrity, the exact methods of business and of prevention of robbery and waste which did so much in that war, I now bid you each and all farewell, with hearty wishes for your continued prosperity and honor."

A few months after his retirement General Meigs received from Congress a notable indication of the confidence and respect entertained for him by that body. When by act approved August 7, 1882, appropriation was made for the erection of the Pension Office building at Washington, it was specially provided that it should be erected "under the supervision of General M. C. Meigs, late Quartermaster General, United States Army, retired," and upon such Government reservation "as may be selected by the Secretary of the Interior, the Secretary of War, and General M. C. Meigs, subject to the approval of the President." Such legislation is extremely unusual.

The Pension Office building was General Meigs' last work, and he took an absorbing interest in its details. Whatever may be

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thought of the architectural beauty of its exterior, presenting as it does a marked innovation upon the style of other public buildings in the District, no one, I believe, will question its fitness for the purpose intended or the economy of its construction.

Social life in Washington in the days before the war was informal and very pleasant. The scientific men of the city, of whom many were widely known throughout the country for their researches and attainments, associated themselves together and organized what has since become the Washington Philosophical Society. Captain Meigs was one of the original members, and at the meetings held weekly in rotation at the several residences of the associates he formed acquaintances and cultivated tastes which influenced his whole future life.

He was elected a member of the American Philosophical Society at Philadelphia, his early home, in 1854, and became member of the Military Order of the Loyal Legion in 1865. He was also associated with several other societies.

In 1841 he married Miss Louisa Rodgers, daughter of Commodore John Rodgers and sister of Admiral John Rodgers, U. S. Navy. Seven children were born to them, four sons and three daughters. Three died in infancy. The others survive their parents, except the oldest son, John R. Meigs, who was graduated at the Military Academy at West Point at the head of his class in June, 1863, and was at once appointed first lieutenant in the Corps of Engineers. After brilliant military services in that corps, for which he was twice brevetted, he was killed in a skirmish with guerillas near Harrisonburg, Virginia, on October 3, 1864, at the early age of 22 years.

In his home life General Meigs was endeared to his family and friends by many traits of gentleness, simplicity, and affection. When relieved from the cares of his public responsibilities he devoted much time to professional reading and study, and, being gifted with a retentive memory, he became possessed of a vast store of information on many subjects.

He died, after a short illness, at his home in Washington, on January 2, 1892. His remains were buried at Arlington, with military honors, on the crest of the hill overlooking the city and the Potomac. The sarcophagus was designed by himself several years before his death.

NATIONAL ACADEMY OF SCIENCES.

In the obituary order published from the headquarters of the army is given the following tribute to his services and character :

“General Meigs was personally a man of kind and amiable character, of strict probity and sense of right, and of great breadth of intellect. The army has rarely possessed an officer who combined within himself so many and valuable attainments and who was entrusted by the Government with a greater variety of weighty responsibilities or who has proved himself more worthy of confidence. There are few whose character and career can be more justly commended or whose lives are more worthy of respect, admiration, and emulation.”

MEMOIR
OF
JULIUS ERASMUS HILGARD.
1825-1890.

BY
E. W. HILGARD.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1893.

MEMOIR OF JULIUS ERASMUS HILGARD.

Julius Erasmus Hilgard was born on January 7, 1825, at Zweibrücken, Rhenish Bavaria, where his father, Theodore Erasmus, held for a number of years the position of chief justice of the court of appeals (supreme court) of that province of Bavaria. Born in the initial year of the French revolution and educated under the influence of that remarkable period, the father was a prominent Liberal in the most liberal province of Germany, and, chafing under the reactionary tendencies and measures that had become especially rampant after the futile attempt to overthrow them, made by some of the hot-headed youth at Frankfort-on-the-Main in 1833, he conceived the purpose of transplanting his large family to the United States, where he hoped to find the "ideal republic" realized. Despite brilliant offers of advancement from the government, he carried out this purpose in the autumn of 1835, accompanied by numerous testimonials of regret and esteem from his constituents, and carrying a letter from General Lafayette commending him to the good offices of the American people.

At that time the subject of this sketch had nearly completed his tenth year and had gone through the third grade of the gymnasium with so much credit that at the closing of the school course preceding his departure he carried off all the prizes—three in number—which were bestowed upon him by the noted educator, Thiersch, according to custom, amid sound of trumpets.

On the voyage from his home at Zweibrücken to the port of Havre, which was made by the only mode of conveyance then known; viz., by wagons, Julius, as the oldest brother among nine children, was first called upon to exercise those practical qualities that served him so well in after life. Landing at New Orleans on Christmas day, after a long passage of 62 days, the family proceeded up the Mississippi, which was running with heavy ice, to St. Louis, and thence to Belleville, Illinois, where the father purchased a farm already well developed, which remained the family residence for many years.

Schools at that time were few and primitive in that region ; hence Julius, together with his brothers and sisters, received instruction from the father, the older ones also teaching the younger ; all, however, with many interruptions from participation in the work of farm and household, inevitable in pioneer life. Of this Julius naturally had his full share, but he varied the routine by devoting himself successively to the study of music, of chemistry and other branches of natural science, while pursuing with his father, by preference, that of ancient and modern languages and mathematics. In the latter he soon outstripped his teacher, and thereafter continued it into the higher branches without outside help, until, in 1843, at the age of 18 years, he went to Philadelphia (where a married sister, Mrs. Sharon Tyndale,* then resided) with a view to the study of engineering and practical employment.

At Philadelphia he soon formed the acquaintance of several prominent families, among them those of Judges Patterson and Kane. At the house of the latter he was first introduced to Professor Bache, who had not long before succeeded Hassler in the superintendency of the United States Coast Survey and was a friend of Hilgard's maternal uncle, August Ritter von Pauli, of Munich, a distinguished engineer.

Another outcome of this connection was a lifelong friendship with Elisha Kent Kane, one of the sons of the family, whose subsequent explorations in the Arctic regions enlisted his hearty sympathy and support.

His first practical employment was in the preliminary surveys of the Bear Mountain railroad, then a new enterprise, but his mental activity in a higher sphere soon manifested itself in a communication to Mr. Bache, made in January, 1844, in which he called attention to errors in the formulas used in the Coast Survey in the computation of geographical positions, and gave his own development of correct formulas. In reply Mr. Bache wrote to the young man of nineteen : " You have overridden two of our most experienced computers and have shown that they are seriously in error." The incident was soon followed up by Bache with an offer of subordinate position in the Coast Survey, in accepting which young Hilgard remarked that he would rather " do high work at low pay than low work at high pay." He entered at once upon the work

* Sharon Tyndale was later on secretary of the State of Illinois.

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assigned him in the computing division, but his official appointment bears the date of December 28, 1846. In the year 1845-'46, however, we already find him engaged in the secondary triangulation of the Chesapeake, below Kent island, the illness of the chief of the party, Captain (since General, C. S. A.) Joseph E. Johnston, left Hilgard in charge of the work. Concerning this first trial of his strength the superintendent's report of that year says: "The measurement of the angles was made by Mr. J. E. Hilgard, whose zeal and the ability with which he has discharged these duties deserve notice here, as they have received it in the reports of the chief of his party."

Among the field-work of the following year was the measurement of the Dauphin island base, made by Superintendent Bache, assisted by Hilgard. The latter then executed the secondary triangulation from Dauphin island to Vermillion bay, amid great difficulties arising from want of suitable means of transportation, and continuing the work late in the season "under conditions of considerable hardship that might have excused him from keeping the field."

The succeeding year (1847-'48) he extended the secondary triangulation of Mississippi sound westward to Lake Borgne, and made observations for latitude and longitude at Dollar point, Galveston bay, including chronometer difference with Pascagoula station. In conjunction with Messrs. Fauntleroy and Davidson, he commenced the measurement of base and main triangulation of Galveston bay.

During all this time, when not in the field he was engaged in computations and investigations at the office, largely under the immediate direction of the superintendent.

In August, 1848, he was married at Washington to Miss Katherine Clements, of that city.

During 1848-'49 he was chiefly occupied in astronomical, magnetic, and latitude observations on the Florida Keys, as well as measurements of a preliminary base and triangulation near Key West and Bahia Monda, and from Key Biscayne towards Carysfoot reef. Again his work was carried on far into the winter season under adverse conditions, and in December and January, 1849-'50, we find him engaged in geodetic and magnetic observations at and off Cape Florida. After some summer field-work on the coast of New England, he returned in September to take charge of the com-

puting department of the Coast Survey office, in which he continued through the years 1851-'52-'53. The efficient condition of that department during that time is specially referred to by the assistants in charge of the office, Captain Benham and Major Stevens, the latter saying: "It affords me unusual gratification to acknowledge the services of Mr. Hilgard, in charge of the computing department, and to commend the zeal, promptitude, and accuracy of every member of that department."

After 1853, while still continuing to have the supervision of the computing department, he was frequently detailed for work of special importance or difficulty, such as the measurement of bases, the testing and discussion of the errors of instruments, etc. On the 31st of March, 1854, while engaged in work on the Dauphin island base, the schooner *Phoenix*, with the entire party on board, was capsize in a squall and sunk to the topmast, on which the party took refuge, afterward escaping in a boat at considerable peril of their lives. Among his work of this year was the perfecting of a reflecting signal vane revolved by the wind, very simple and effective, and extensively used since.

In 1855, while still continuing occasional field-work, he was put in charge of the publication of the records and results of the Coast Survey, in order to insure a steady progress in publication. This, together with miscellaneous field and scientific work, including a series of very elaborate experiments on the comparison of the standard bar of the base apparatus with the standard meter, occupied his time very closely up to the middle of the year 1860, when the necessary attention to his material interests led him to dissolve his official connection with the Coast Survey for a time, in order to engage in a prominent business enterprise at Paterson, New Jersey.

He found himself, however, unable to relinquish his interest in the affairs of his "first love," the Coast Survey, and when, at the outbreak of the civil war, Superintendent Bache appealed to him to return and lend his aid in supporting the Survey, when its very existence was threatened as a measure of ill-understood economy, he promptly dissolved his lucrative business connection and repaired to Washington, where he brought strong arguments to bear upon Schuyler Colfax and Roscoe Conkling (eminent as leaders in the dominant party), showing how important such an organization as the Coast Survey must be to the country in time of war. At first, Conkling's close and critical examination of his argument led Hil-

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gard to believe him actuated by a spirit hostile to the Survey; but he was greatly relieved to find, soon afterward, that he had converted both men into earnest and active supporters of the work. Subsequently he took an active part in organizing the National Academy of Sciences, of which he became a charter member; he also contributed largely to the investigations made by that body for the Government.

In 1862 he assumed charge of the Coast Survey office, a position which during the war involved heavy responsibilities, which were soon aggravated by the beginning of the mental disease that incapacitated Mr. Bache for performance of the duties of Superintendent, and terminated in his death. Gradually Hilgard found himself forced to assume, one after another, the duties of Superintendent, until in the autumn of 1864 the whole devolved upon him, in addition to his duties as assistant in charge of the office. In this onerous and responsible position he remained, under heavy strain, until, in February, 1867, the appointment of Benjamin Peirce as Superintendent terminated the interregnum. All these extra services were rendered by him without compensation.

Of the political causes which brought about Hilgard's being passed over in the appointment of a successor to Bache it would be profitless to speak here. As the one who had carried the burden of the work during the most critical period, and had not only brought it through safely, but with increased reputation and appreciation for efficiency and usefulness, his claims were too obvious to be overlooked or fairly contested. As to his action under the slight, it is pertinent to introduce the following testimony of Superintendent Peirce, a competent witness, who cannot be suspected of partiality:

"During the illness of my lamented predecessor the administration of the Survey fell upon the shoulders of the assistant in charge, Mr. J. E. Hilgard. The distinguished ability with which this difficult service was discharged was manifest to all. He has extended to me the benefit of this experience liberally and loyally. While I willingly acknowledge myself under deep and lasting obligations to him for the aid thus rendered me, I can also testify that in all respects he has been equally true to my predecessor, the greatness of whose reputation has not been diminished in his keeping."

That, with such a record and with such testimony in his favor, his claims should have been passed over a second time in the appoint-

ment of the successor of Peirce, on the resignation of the latter, was not unexpected, in view of the continuation in power of the same dominant influences; that the event should have passed without exciting some painful reflections with him and others cognizant of the facts would have been unnatural. It was partly, doubtless, under the influence of a feeling that reparation was due that Hilgard was elected to the presidency of the American Association for the Advancement of Science for the year 1875.

He, however, continued in charge of the Coast Survey office and in the performance of a great variety of scientific work until his appointment to the superintendency in 1881. The duty of conducting the work of the office of Weights and Measures also devolved upon him as assistant superintendent during that time, and he was essentially active in shaping the legislation concerning the legalization of the metric system in the United States. The metric standards for the States were prepared under his supervision.

In April, 1872, he departed for Europe in charge of operations for the telegraphic determination of transatlantic longitude, specially between the observatories of Greenwich and Paris on the one hand, and those of Harvard and Washington on the other. The successful accomplishment of this long-pending task by himself as an American was a source of much gratification to him.

He returned to his duties at the Coast Survey office in November, 1872, and in the course of that year made a special determination of the length of the standard bar used in the measurement of the primary base line near Atlanta, Ga. The following year he continued the same duties until September, 1874, when he departed for Europe a second time, under an appointment as a delegate to represent the Government at the International Conference for the construction of a new standard meter, which had been called at Paris. The report of the Superintendent says on this subject:

"To the watchful care of Professor Hilgard for the interest of the public service and his intimate relations with leading scientific men at home and abroad is mainly due the present standing of the office. As a member of the International Committee on Weights and Measures, to which body he has been accredited by the President of the United States, Mr. Hilgard, as vice-president of the commission, will take passage for Europe before the close of the present month to attend a meeting of the body at Paris."

He participated actively in its deliberations with such ability that when it had been determined to establish an International Bureau of Weights and Measures at Paris, the directorship of the new institution was offered to him, but was declined, to the surprise and regret of many of his friends who were familiar with the aspect of affairs at Washington, and who would have desired for him the *otium cum dignitate* of such a position.

In a subsequent report, the Superintendent of the Coast Survey says, regarding this offer: "The invitation tendered to Assistant Hilgard to accept the directorship of the bureau marks the sense held in that eminently scientific body of this special fitness for organizing and conducting an institution so exacting in its scientific demands and so novel in political inception. While declining the proffered honor, he will nevertheless continue his coöperation as a member of the International Committee on Weights and Measures."

Subsequently a beautiful Sèvres china vase was presented to him on behalf of the French Government by President Thiers, in recognition of his services in connection with the International Commission, a similar one being presented to Professor Joseph Henry.

In the report of 1874-'75, the Superintendent remarks in respect to Hilgard's relations to the Survey: "To his long experience, eminent ability, and thorough comprehension of the requirements of the work, is due the effective coöperation of the office work with that in the field."

In the report of the following year we find a paper from his pen, on a chart of the magnetic declination of the United States.

In 1875 he wrote a paper for the American Association for the Advancement of Science "On the Measurement of a Base Line for the Primary Triangulation of the United States Coast Survey near Atlanta, Georgia;" another for the Philosophical Society of Washington on "The Relation of the Legal Standards of Measure of the United States to those of Great Britain and France." In August, 1876, he also delivered an address as retiring president of the American Association for the Advancement of Science.

During the following year, while continuing in charge of the office, he was appointed Inspector of Standard Weights and Measures, sets of which had been ordered to be distributed to the several states. He also made a comparison of the American and English standard yards, a matter of great importance and interest. He

likewise described an optical densimeter for sea water,* and made important instrumental comparisons. The report of the Superintendent for 1876-'77 says :

"In the summer of 1876, but without remitting in executive duties pertaining to the Coast Survey office, Mr. Hilgard acted as one of the judges on scientific and mechanical apparatus at the Centennial Exposition at Philadelphia. His intimate knowledge respecting instruments of precision associated him, as a leading member of the board of judges, with some of the ablest scientists of America and Europe.

"Subsequently, on invitation, he delivered a course of 20 lectures for the Johns Hopkins University at Baltimore, on the subject of 'Extended Territorial Surveying,' thus contributing the benefit of his long experience to the training of young men for a branch of work which is one of the great needs of the country—an accurate mapping of its entire surface."

In June, 1878, he was detailed from his office-work for important duties requiring the presence of a representative of the Coast Survey in Europe. He assisted in organizing an International Committee on Weights and Measures; made at London a comparison of the Coast Survey standard yard with the British Imperial standard, and also attended the annual session of the International Geodetic Association at Hamburg.

On his return from Europe, in November, 1878, he resumed charge of the office, directing many scientific investigations and, among others, selecting points for magnetic observations in the United States from and in addition to those at which observations had for a number of years been made at the charge of the "Bache fund" under his direction. He made records and reports of these observations to the National Academy of Sciences in 1881.

In December of that year, on the death of Superintendent Patterson, he was himself appointed to the office and continued in the same until, in July, 1885, he was obliged to resign. Among the special work done by him during that period was the construction of a relief model of the basin of the Gulf of Mexico (for which he suggested the name of the "Bay of North America"), and the publication of a memoir on the same, published in the *American Journal of Science*.

*For this instrument a gold medal was subsequently awarded to him at the London International Fisheries Exposition, 1883.

It is certain that at the time of his appointment to the superintendency his physical and mental strength had already become seriously impaired by the inroads of the insidious disease (Bright's) which ultimately, after several years of often intense suffering, ended in his death. That to some extent he was conscious of his enfeebled condition is evidenced by his remark when receiving the notice of his appointment, that "it came too late." Years before his life had been saddened by the loss of three young children; it was now darkened by the death of the last remaining son, Frederic, who, though always delicate, had lived to manhood and appeared to be entering upon a successful career as civil engineer. This blow, coming at a time when even in his letters he complained of increasing inability to do the accustomed amount of work, gave a grave downward impulse to his already failing strength. That he did not at that time find any one to lean upon as Bache had leaned upon him under similar circumstances was a misfortune which allowed matters to progress until a political change found in his case a welcome opportunity for the exhibition of the reformer's zeal. In respect to his physical and consequent mental condition at the time when he appeared before the Presidential commission on the conduct of the Coast Survey, it may suffice to say that the physician who examined him immediately after his resignation declared that had he been aware of the facts he "would, unsolicited and from a sense of justice, have gone before that body and testified that Hilgard could not be held responsible for his acts." This statement, amply corroborated by the severe illness immediately following, should dispose of the unjust censure visited upon him at the time, whether by those ignorant or by others forgetful of the devotion, singleness of purpose, and high ability which had uniformly marked his previous career. While there could be no question of the necessity for a change, the difficulty long experienced by the administration in finding a successor among his peers in science stands in evidence of the feeling excited in them by the harshness of the treatment bestowed upon him.

In his personal and social relations, Hilgard was early noted for his geniality, an intense appreciation of the beautiful in art and literature, and a thorough enjoyment of the humorous, however grave might be the situation that gave rise to it. He remained through life very fond of the ancient classics. A small pocket edition volume of Horace's odes long accompanied him on his travels, and he

was fond of quoting for his own and others' benefit on critical occasions the "*Æquam memento rebus in arduis servare mentem*," a principle he honored by close observance so long as he remained of sound mind. Perhaps none of his personal traits was more pronounced than his generosity toward those in need, whether of substantial aid (which was frequently given beyond his financial ability) or of advice and moral support. To deserving young men especially he liked to extend a helping hand, and tired not of doing so, even though too often illy rewarded for his efforts. He often said that the older men ought to see to it that the right kind of successors were ready to fill their places when they left the stage. The loyal friendship he bore toward others he trusted would be reciprocally extended, and he seemingly preferred the disappointment thus frequently incurred to a relinquishment of his faith in human nature at large. That the life of a mind so constituted should be largely checkered with disappointment was inevitable; but to such lives the unswervingly faithful performance of duty, the singleness of high purpose, and the accomplishment of the same, so far as circumstances permit, carry their own reward.

He died at his residence in Washington, after a lingering and painful illness, on May 9, 1890.

MEMOIR
OF
AMOS HENRY WORTHEN.
1813-1888.

BY
CHARLES A. WHITE.

READ BEFORE THE NATIONAL ACADEMY, NOVEMBER, 1893.

BIOGRAPHICAL MEMOIR OF AMOS H. WORTHEN.

Mr. President and Members of the Academy:

In compliance with the requirement which I understand to be implied by my appointment to write a biographical memoir of our deceased associate, Mr. Amos H. Worthen, I have prepared a short narrative of those events and incidents of his life which reveal the characteristics of the man, together with a statement of the principal facts which relate to his scientific career. The latter I have based upon his published works and my own personal recollections, and the former upon information received from his surviving kindred.

Amos Henry Worthen was born in the town of Bradford, Orange county, Vermont, October 31, 1813. He was a son of Thomas Worthen and Susannah Adams, his wife, and the eleventh of the twelve children born to them, all of whom are now dead.

Of the father, Thomas Worthen, I have been able to learn comparatively little, but he seems to have come of a Massachusetts family, descendants of which are scattered through a number of the States of the Union. In early manhood he came with his young wife to Bradford and purchased a farm upon what was known as the South Road, a few miles westward from Bradford village, where they made their permanent home and where their children were born and reared. He was born August 24, 1765, and died October 21, 1851.

His wife Susannah was born December 24, 1768, and died March 17, 1843. She was the eldest child of Abraham Adams, a descendant of Henry Adams, who came from England and settled at Mount Wollaston (now Quincy), Massachusetts, and who was the founder in America of the celebrated Adams family that has included two Presidents of the United States. She was a woman of great energy and tact, and is remembered by her descendants as possessing a most amiable disposition. It is evident that it was largely from her that her son Amos inherited the force of character and adherence to a definite purpose which distinguished him. Between mother and son also there was not only the strong natural affection which such a relation engenders, but during the whole of their concurrent life there was the closest sympathy between them.

The childhood and early youth of Amos were spent continuously upon the farm, where he was employed during the warmer months of the year in such of its labors as were suitable to his strength, but in winter these labors were sufficiently respite to allow him to attend the district schools of the neighborhood. His well-directed labor and the sports which the surrounding fields and forests afforded resulted in the development of a vigorous physical manhood, and his winter schooling—for the Vermont district schools were among the best of their class—gave him the foundation of that mental development that afterward distinguished him. As his youth progressed and he had mastered the rudiments taught in the district schools he desired to enter Bradford Academy, since merged with the Bradford High School, which was located at the village a few miles from his home. He obtained the means to gratify his wish, and it was at this institution that young Worthen received the last of his school training.

During his attendance at Bradford Academy he boarded in the family of his sister Mary, who was older than himself and who had married Captain Ellis Bliss, an extensive farmer in the Connecticut valley. It is mainly to N. W. Bliss, Esq., of Chicago, one of the sons of this gentleman and a fellow-pupil at the Academy with his youthful uncle, that I am indebted for the larger part of these details of his earlier personal history.

While prosecuting his studies at the Academy young Worthen also prosecuted his suit for the hand of Miss Sarah B. Kimball, of Warren, New Hampshire, and they were married on January 14, 1834, at which time he was in his twenty-first year. The union was a fortunate one and lasted fifty-three years, when it was broken by the death of his faithful wife, which occurred only a little more than a year previous to his own. Seven children were born to them—one daughter and six sons. The daughter died in childhood, but all the sons still survive him.

Upon assuming his new responsibilities the young husband decided to make his home in what was then called the Far West, Ohio and Kentucky, with few exceptions, receiving the most adventurous of the emigrants from New England. Such a journey was then a serious undertaking, most of it being through a comparative wilderness and by slow means of transportation, for railroads were yet in an experimental condition and none of the few then existing was available for any portion of it. His eldest brother,

Enoch, had already removed with his family to Cynthiana, Kentucky, and thither Amos also decided to go. His stay at Cynthiana seems to have been short, for we learn that in the next year after leaving his New England home he was teaching school at Cumminsville, near Cincinnati, Ohio. He remained at Cumminsville until June, 1836, when he joined the tide of emigration which had begun to flow into the Mississippi valley, and settled at Warsaw, Illinois. Here, with the exception of an interval of two years, 1842 to 1844, which he, together with his family, spent in Charlestown, Massachusetts, he lived continuously until his death.

Some of his wife's family, the Kimballs, had preceded the young couple to Warsaw and its vicinity, and with one of her brothers Mr. Worthen formed a partnership in mercantile business. This business, with the exception of several changes and interruptions, he continued until 1855, after which time he devoted himself entirely to the scientific pursuits for which he had been preparing himself ever since his arrival in Warsaw.

It is a fact, but not a surprising one, that Mr. Worthen's career as a merchant was not successful as success is usually counted in such occupations. Business as such had no attractions for him, or none beyond the fact that it yielded him the moderate means of meeting the necessary demands of his growing family, but unconsciously to his friends, who were sometimes inclined to censure him for what to them was an evident lack of business enterprise, it was giving him an opportunity to accomplish far more than this. His inherent predilection for natural science had from his boyhood been subdued or kept latent by the pressure of duties to the demands of which he was ever ready to respond, but it found at his new home and in its vicinity an unwonted and irresistible stimulation.

The forests of the Mississippi valley and the adjacent broad prairies afforded him a boundless field for study and observation, and, above all, the fossiliferous rocks of the Lower Carboniferous series, which prevail in that region and some of which are well exposed at and around his home, were his especial delight. He began at once the study of this great series of strata and the collection of its fossils and minerals. With these as a basis he soon established a system of exchanges with correspondents in the East by which he received in return such books and mineralogical and zoölogical specimens as were necessary in the prosecution of his studies.

This work was undertaken more than fifty years ago, and it is

well to refer to some of the conditions then existing which are very different from those which now exist—for example, postage on each of his letters of correspondence cost 25 cents, a not inconsiderable sum in those days, when the country was drained of its money to pay for Government lands. Because there were then no overland freight or express lines, all his exchanges were necessarily sent and received by Mississippi river steamers between Warsaw and New Orleans, and by Gulf and Atlantic sailing vessels between that city and Boston; therefore many months would often elapse before getting returns from his sendings. Moreover, an examination of the dates of publication of the books which are now indispensable in the elementary study of geology, and even of those which preceded them, will reveal the fact that none of them was then in existence, and reference to the curricula of the best American colleges then existing will show that no better instruction was available for any American student than that which this self-taught naturalist was then obtaining for himself. These are conditions the difficulties of which the younger naturalists of to-day, who have had the advantage of special training in scientific schools and who have free access to great libraries and direct communication with a multitude of scientific workers, cannot well understand.

Mr. Worthen made frequent journeys in the region traversed by the upper Mississippi river and brought together large collections of fossils; his place of business and his home became the one a storehouse and the other the workshop of a naturalist, and the result of it all was that while his knowledge increased, business languished. Still this incongruous relation was continued until about the year 1853, and it was not permanently broken until 1855, when he abandoned mercantile business and devoted himself entirely to his scientific pursuits.

In 1851 he began attendance upon the meetings of the American Association for the Advancement of Science, in which he continued his membership until his death. By this and other means he extended his acquaintance with the geologists of the whole country, so that when the time came he was able to make an intelligent selection of assistants for his public work.

At that early date he had made himself master of the general geological features of that portion of the Mississippi valley which borders upon Illinois, and when, in 1853, he was invited by Dr. J. G. Norwood, then State Geologist, to assist him in his official

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work, he left his business temporarily for that purpose. This was his first public work, but its results were never published except as they were subsequently embodied in his own official reports.

In 1855 he was appointed assistant to Professor James Hall, then State Geologist of Iowa, and he continued upon that work until the close of 1857. Besides contributing two important chapters to Professor Hall's report upon the geology of that State, he aided him largely in constructing the geological section along the Mississippi river from Lansing, Iowa, to St. Louis, Missouri, which is published in that report. It was while engaged upon the construction of this section that my personal acquaintance with Mr. Worthen began, and which was continued with great gratification to myself during the remainder of his life.

On March 22, 1858, the State geological work of Illinois having previously become suspended, Governor Bissell commissioned Mr. Worthen as State Geologist, and he at once assumed the duties of that office. His previous long period of patient labor had been preparatory, but his entrance upon this public office was the beginning of a career upon the like of which he had long ardently desired to enter and which he prosecuted without interruption until his death, which did not occur until thirty years afterward.

Upon assuming the duties of his office he quickly comprehended the magnitude of the task that lay before him, and he began to gather about him men skilled in various branches of scientific work, the names of six of whom appear upon the list of either living or deceased members of this Academy. The following is a list of those who have aided him in this work and who have contributed their writings to his reports and other publications:

Bannister, H. M.	Lindahl, Josua
Barris, W. H.	Meek, F. B.
Blaney, J. V. Z.	Miller, S. A.
Bradley, F. H.	Newberry, J. S.
Broadhead, G. C.	Prout, H. A.
Cope, E. D.	Scudder, S. H.
Cox, E. T.	Shaw, James
Engelmann, Henry	Springer, Frank
Everett, Oliver	St. John, Orestes
Freeman, H. C.	Ulrich, E. O.
Green, H. A.	Wachsmuth, Charles
Lesquereux, Leo	Whitney, J. D.

From and after the time he entered upon his duties as State Geologist of Illinois, the current of his life, although extending through thirty years, was too uniform to require a material extension of this biographical notice. During all that time his labor was constant and severe, and he took few vacations beyond his attendance upon scientific meetings, but his great physical strength made him equal to his self-imposed tasks. His delight in his work and in his home were such that his share of happiness was greater than that which falls to the lot of most men, and he desired no addition to it.

Although the current of the latter part of his life was comparatively uniform, it was made so by the same energy that had before made it eventful—that is, it was the result of his persistent adherence to a settled purpose and his consummate tact in so managing the affairs of his office as to insure the continuance of his work. There might have been other opportunities of a similar kind which offered equal, if not greater, promise of success than his own, but he did not covet them. He might have extended his investigations into other fields, but he chose to confine himself mainly to the State of Illinois. He had reached the climax of his ambition so far as opportunity was concerned, and he deliberately devoted all the energies of the remainder of his life to the fulfillment of his plans, and from this purpose he never swerved.

But his continued success was not due to the absence of adverse conditions. Again and again his work was in danger of suspension by the threatened failure of the necessary appropriations by the legislature, and more than once they were so far reduced that only the most careful management averted disaster. Once, indeed, appropriations failed entirely,* but he continued his work without compensation and with such evident sincerity of purpose that they were resumed by the next legislature. One by one his volumes of reports were published and gladly accepted by the scientific world; one by one the great difficulties he encountered were overcome, and he was permitted to finish his work to the great advantage of science and the satisfaction of the most exacting economist.

* The omitted appropriation was for the years 1875 to 1877. When appropriations were resumed in the last-named year provision was made for the establishment of the Illinois State Historical Library and Natural History Museum, and Mr. Worthen became its curator as well as State Geologist.

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As his work approached completion old age was coming upon him, but he retained his mental vigor, and in a great measure his accustomed good health, until his final sickness, which prostrated him suddenly and terminated in death within a few days. He died on Sunday, May 6, 1888, in the 75th year of his age, surrounded by the people who respected and honored him and among whom he had lived a full half century. *

Personally, Mr. Worthen was of manly presence and kindly, candid, and unpretentious in manner. He was impulsively generous to his friends and charitable, even to those with whom he had little sympathy, but he was uncompromising in his love of justice and scientific truth.

The only proper standard by which to judge of his scientific labors is, and must always be, his published works, the list of which follows. These are his monument and the records by which his name will be known in future years. Extended comment upon them is not necessary upon this occasion, because more than twenty years ago this Academy carefully investigated the results of his labors and decided the question of their merit in his favor by electing him to its membership. This was done when only four of the eight volumes of his official reports were published, but it is at least safe to say that his later was in no way inferior to his earlier work.

* His six sons, Fay S., George B., Thomas A., Amos H., Jr., Charles K., and John B., were the pall-bearers at their father's funeral. His body lies buried in Oakland cemetery, two miles south of his former home in Warsaw. The place of his burial is marked by a granite family monument about seven feet in height and bearing six inscription-panels. Of the two panels upon the east side of the monument one bears his wife's name and date of her birth and death, and the other bears the following inscription :

AMOS H. WORTHEN.

Oct. 31st, 1813.

May 6th, 1888.

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33. A. H. WORTHEN.

Descriptions of fifty-four new species of Crinoids from the Lower Carboniferous Limestones and Coal Measures of Illinois and Iowa. In Bulletin No. I, Illinois State Museum of Natural History, pp. 3-38, 1882.

34. A. H. WORTHEN.

Corrections and proposed new names for species previously described in Geological Survey of Illinois under names that were pre-occupied; and descriptions of two new species of fossil shells from the Coal Measures of Illinois and Kansas. In Bull. No. I, Illinois State Museum of Natural History, pp. 38-40.

35. A. H. WORTHEN.

Economic Geology of Illinois. Reprinted from the Original reports of the Geological Survey, with additions and emendations. Published by authority of the Legislature of Illinois. Springfield, 1882. Vol. I, pp. 541; Vol. II, pp. 615; Vol. III, pp. 596.

36. Geological Survey of Illinois, A. H. WORTHEN, Director; Vol. VII, Geology and Paleontology, pp. i-iv and 1-373, plates I-XXXI. Imp. 8vo, Chicago, 1883.

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37. A. H. WORTHEN.

Bulletin No. 2 of the Illinois State Museum of Natural History. Descriptions of two new species of Crustacea, fifty-one species of Mollusca, and three species of Crinoids, from the Carboniferous formation of Illinois and adjacent States, pp. 27. Imp. 8vo, Springfield, March, 1884.

38. Geological Survey of Illinois, A. H. WORTHEN, Director; Vol. VIII, Geology and Palæontology, pp. i-xi and 1-728, plates I-LXXVIII. Posthumous publication, edited by Josua Lindahl, with an appendix, pp. 1-151, and map. Imp. 8vo, Springfield, 1890.

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The foregoing list comprises all the publications of Mr. Worthen that have come to my knowledge, and it is believed to be nearly or quite complete. It is proper to say that most, if not all, of the matter contained in the publications mentioned in that list, exclusive of the eight large volumes of survey reports, is also contained in those volumes in a completed form—that is, those publications consist of material which was printed in advance of the reports, or of that which has been abstracted from those volumes after their publication. The eight large volumes and accompanying maps therefore, represent fairly the results of Mr. Worthen's life-work.

This great work embraces the geology, both structural and economic, and the palæontology of Illinois. In the geological division was accomplished the mapping, upon one comprehensive sheet and various smaller ones, of the nearly 56,000 square miles which are embraced within the boundaries of that State, in such a way as to show its geological structure and the area occupied by each formation. The geological structure is further shown by numerous measured sections, most of them showing local relations of the strata, but the principal one was constructed from exposures along the Mississippi river, which forms the whole western boundary of the State. This section shows the relation of the different formations, from the Potsdam sandstone to the Coal Measures, inclusive, for a distance of more than 600 miles.

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The immense economic resources of the State are also shown in these volumes. The characteristics and outlines of its 37,000 square miles of productive coal-field are recorded. The lead region has been carefully explored and mapped, and the character of its mineral products determined. Besides this, the character of the soils of the State is described, and its resources in stone and other building material are shown to be abundant and good.

The most conspicuous portion of this great work, however, is its palæontology. This portion is so extensive and elaborate that the volumes of the Illinois survey have long been indispensable to every palæontologist in every part of the world where the palæozoic formations are studied. The fossil forms described are all well illustrated. They have been collected from all the formations, from the Potsdam Sandstone to the Upper Coal Measures, inclusive; but those of the Lower Carboniferous and the Coal Measures are especially abundant. Some indication of the extent of this palæontological work is shown by the fact that it embraces the description and illustration of 297 species of vertebrates, 1,073 species of invertebrates, and 256 species of plants. Nearly 1,500 of these species were first made known to the scientific world through the publications enumerated in the foregoing list.

MEMOIR
OF
WILLIAM P. TROWBRIDGE.
1825-1892.

BY
C. B. COMSTOCK.

READ BEFORE THE NATIONAL ACADEMY, NOVEMBER, 1893.

BIOGRAPHICAL MEMOIR OF WILLIAM P. TROWBRIDGE.

The National Academy has lost another of its members, one who was devoted to applied science, which in the last seventy years has so rapidly changed the whole of civilized activity, and who, being in contact with that vast field on many sides, was intellectually a well-rounded man.

Prof. William P. Trowbridge was born in Troy, Michigan, on May 25, 1825, and died at New Haven, Connecticut, on August 12, 1892.

With a father in the war of 1812, with a grandfather who as a boy took part in the battle of Lexington and was afterward a brevet captain in the Continental army, he came of stock from which it is an honor to descend and which has made so strong an impression on our national life.

Of the way in which characters and traits are transmitted we know nothing; of the fact of transmission no one can doubt. A friend of Professor Trowbridge's father saw in a shop window in London a portrait, as he thought, of his friend. On examination it proved to be that of Sir Thomas Trowbridge, of the Royal navy, who fought with Nelson. There was doubtless a distant connection, and over the long route the type had been retained.

Professor Trowbridge was appointed a cadet at the United States Military Academy at West Point, and graduated at the head of his class in 1848, serving in the last year of his cadetship as acting assistant professor of chemistry. Soon after graduation he was ordered back to West Point as an assistant in the astronomical observatory, where he prepared himself for duty on the Coast Survey, on which he served from 1851 to 1856. In this survey he was engaged on the primary triangulation of the coast of Maine; on surveys of the Appomattox and James rivers, recommending on the latter the cut-off at Dutch gap, which was subsequently undertaken by General Butler during the war and has since been finished. From 1853 to 1856 he was occupied on magnetic and tidal observations, extending on the Pacific coast from Puget sound to San Diego, California. In this time he made natural history collections amount-

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ing in all to several thousand specimens, which he subsequently presented to the University of Michigan. He was promoted to second lieutenant, Corps of Engineers, on July 1, 1848; to first lieutenant, Corps of Engineers, December 18, 1854, and resigned from the army on December 1, 1856, to accept the professorship of mathematics at the University of Michigan. Not finding this position what he had expected, he resigned in 1857, and at the solicitation of Professor Bache accepted a position as assistant on the Coast Survey, where he was employed on Gulf stream observations and in magnetic work at Key West. From 1862 to 1865 he was in charge of the engineer agency at New York city, and of the construction of the fort at Willets Point, as well as of repairs to other works in New York harbor.

From 1865 to 1871 he was the vice-president of the Novelty Iron Works at New York city; from 1871 to 1877 the professor of dynamic engineering in the Sheffield Scientific School of Yale College; from 1877 till his death, in 1892, he was the professor of engineering in the School of Mines at Columbia College. In 1880 he took charge, under the Tenth Census, of the department of power and machinery employed in manufactures. In addition to these duties he was the adjutant general of Connecticut from 1872 to 1876; was commissioner for building the capitol at Hartford from 1873 to 1878, and commissioner for establishing harbor lines at New Haven from 1872 to 1878.

He was associate editor with Dr. Barnard of Johnson's *Cyclopedia*. He was the author of "Heat and Heat-engines" (1874); of several memoirs which have appeared in the reports of the Coast Survey; of an early plan for a cantilever bridge across the East river, and of numerous contributions to the *Academy of Sciences of New York*.

The degree of A. M. was conferred on him by the Rochester University in 1856 and by Yale in 1870; that of Ph. D. by Princeton in 1880; that of LL. D. by Trinity in 1883 and by the University of Michigan in 1887.

He was a member of the New York Academy of Science, of the American Association for the Advancement of Science, and was elected a member of the National Academy in 1872.

He was married at Savannah, Georgia, to Miss Lucy Parkman on April 21, 1857, who survives him. Such is a brief synopsis of the principal events in a busy and useful life.

WILLIAM P. TROWBRIDGE.

Thus far only his intellectual activity, wide and varied as that was, has been spoken of. Of his fitness as a teacher one of his associates in Columbia College has said, "He had that rare combination of personal qualities of character with intellectual capacity and technical acquirement which made him an almost ideal selection for the position which he was called upon to fill. With ability to appear before a broader public with credit, and appreciating the advantages which follow to an institution from such wider recognition of its professors, yet withal so modest and conscientious as never to be tempted to neglect a new duty because inconspicuous, his loss is made the more greivous by the very rarity of the union of these qualities in one person."

The intellectual life, valuable as it may be to the world, is largely the result of the activity of a thinking machine working on its proper objects. It is not the man. To know the man one must know the ideals which influence or control his life. In the case of Trowbridge one must also know the loving relations with those near to him that strengthened with the growing years; must know the respect and admiration which his fellows in the long path of life felt for him; must know the goodness they saw sculptured in his face by the slow hand of time.

Religious, unselfish, loving, we may well wish to leave a memory like his that shall—

Smell sweet and blossom in the dust.

MEMOIR
OF
JOHN LE CONTE.
1818-1891.

BY
JOSEPH LE CONTE.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1894.

BIOGRAPHICAL MEMOIR OF JOHN LE CONTE.

John Le Conte was born on a plantation in Liberty county, Georgia, December 4, 1818. As his family name indicates, he was of French Huguenot descent. His earliest American ancestor, Guillaume Le Conte, left his native city of Rouen soon after the revocation of the edict of Nantes, 1685, and in consequence thereof, and after a brief stay in Holland and in England, came to America and settled in the vicinity of New York. A full account of his ancestry has already been given in the biography of his cousin, John Lawrence Le Conte (*Biogr. Mem.*, vol. ii, p. 263), and need not be repeated here. But his immediate parentage and the conditions under which he was born and educated had so important a bearing on his character and life-work that it cannot be passed over without brief mention. It is much to be regretted that we know so little of the boyhood of distinguished men, for this is the character-forming period of life.

Louis Le Conte, the father of John, was the elder brother of Major John Eatton Le Conte, so well known in the history of American science. He was born August 4, 1782, it is believed in Shrewsbury, N. J., but lived and received his early education in the city of New York, and was graduated in Columbia College in 1800, at the early age of eighteen. After graduation he studied medicine with the celebrated Dr. Hosack, but, it is believed, never graduated in that profession. He certainly, however, acquired great knowledge and skill in medicine, which was of great importance to him subsequently on his Georgia plantation. About 1810 he removed to Liberty county, Georgia, to take possession of a large property in land and negroes left him by his father, John.

Liberty county was originally settled by a colony of English Puritans, who have left their strong impress on the character of the people of that county even to the present day. A more intelligent and moral community I have never seen. It received its name of Liberty in recognition of the fact that it was the first colony in Georgia to raise the flag of independence on the breaking out of the war of the Revolution, in 1776.

NATIONAL ACADEMY OF SCIENCES.

Louis Le Conte married Ann Quarterman, a maiden of this colony. Of the seven children, four sons and three daughters, the issue of this marriage, John was the fourth child and the second son. His father, Louis, lived on his plantation and devoted himself entirely to the care and management of his large property and to the passionate pursuit of science in nearly all departments, but especially in those of chemistry and botany, in both of which his knowledge was both extensive and accurate. The large attic of his plantation-house was fitted up as a chemical laboratory, in which he carried on researches daily. I well remember what a privilege it was to us boys to be permitted sometimes to be present, and with what silent awe and tiptoe steps we, especially John, followed him about and watched these mysterious experiments.

His devotion to botany was even, if possible, still more intense. A large area of several acres of enclosed premises was devoted to the maintenance of a botanical and floral garden, widely known at that time as one of the best in the United States, and often visited by botanists, both American and foreign. Far removed from any city (Savannah was near forty miles distant), this garden was used only for scientific study and refined enjoyment. It was the never-ceasing delight of the children. The tenderest memories cluster around it, especially about the image of our father in his daily walks there after breakfast, sipping his last cup of coffee, enjoying its beauty, planning improvements, and directing the labor of the old negro gardener, "Daddy Dick." It is, alas, in ruins now, but some of the grand *camelia japonica* trees, of which there were eight or ten, still remain. I said "trees," for in December, 1891, I visited the old place and measured some of these. The largest, a double white, measured fifty-four inches in girth; ten inches from the ground where the first branches came off. In bygone days I have seen at least one thousand pure white blossoms five inches in diameter and double to the center on it at once.

To supply this garden he made many excursions, often with visiting botanists or collectors, sometimes lasting several days, and always returning laden with botanical treasures. As evidence of his keen perception of the true affinities of plants, it is noteworthy that although the Linnean system was at that time universally used, yet even at this early day he always spoke of the affinities of plants in terms of their natural orders.

Nor was he neglectful of other departments of science. This was

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well shown in the composition of his large library of scientific books and periodicals. In fact, his love of nature was so spontaneous and passionate that it could not but extend in all directions. Mathematics, astronomy, physics, geology, and zoölogy alike engaged his attention. I remember well the intense enthusiasm with which he read Lyell's *Principles of Geology* when first published. I remember, too, his delight in working out the most complex mathematical puzzles; such, for example, as magic squares. The boys were all ardent gunners, but under his influence we never failed to observe carefully what we shot. Every new form of bird or beast was brought home in triumph to be determined in name and affinities by him.

Nor was he wanting in kinds of culture other than scientific. His training in Latin, for example, was so thorough that he read it at sight almost as readily as English.

It is easy to see from the above sketch that Louis Le Conte was one of a type of scholars now almost extinct. Such simple, disinterested love of truth for its own sake, such open-eyed, yet thoughtful, observation in all directions, such keen insight, such passionate love of nature, and all combined with such utter forgetfulness of self and absence of any ambition or vanity of reputation. Those who knew him best, but especially his brother, Major John Le Conte, affirmed that he made many important discoveries in both chemistry and botany, yet he never published a line, but freely gave away his new things in the latter science to his many correspondents in New York.

Here, then, until his death, in 1838, he lived his simple, quiet life of intellectual culture and beneficent activity, administering the affairs of an estate with two hundred slaves with firmness and kindness, daily directing their labor, visiting the sick, and caring for the old. His medical knowledge was of inestimable value to him now, not only on his own place, but to the poor of the surrounding country, who were unable to pay for medical service. His plantation was on the borders of the pine barrens of McIntosh county, inhabited only by a shiftless class of "Pine Knockers." For twenty miles about, in pure charity, he visited these people in their sickness; and in chronic cases; even bringing their children to his own house, as the only hope of their recovery. In order to diminish their sense of dependence and to cultivate in them, if possible, a sense of self-respect he sometimes required of them in return some light work,

as picking of cotton or gathering of corn. He was looked up to by these poor people as a being of another order from themselves.

It is easy to imagine the passionate love, the reverence, approaching to fear and even to worship, with which he inspired his children. The effect of such a life and such a character on young John is simply inestimable. To the day of his death John looked back on his father with the greatest love and reverence and upon his influence as the greatest of all influences in forming his character; and, indeed, of all the children John most resembled his father.

I have dwelt somewhat on the life and character of Louis Le Conte, not only because of its paramount influence on his children, especially John, but also because such a life and such a character ought not to go wholly unrecorded.

Liberty county at that time abounded in game of all kinds and its waters swarmed with fish. The mother died early (John was then eight years old), and the boys were left wholly in the care of the father. His theory of the education of boys was to give as much freedom as was at all consistent with safety. He allowed us the free use of fire-arms, but early impressed upon us the habit of careful handling. All the boys were, of course, passionately fond of field sports of all kinds. Indeed, life on a plantation in the South at that time was a very paradise for boys. Not only the unlimited hunting, fishing, swimming, &c., but all the multitudinous farm operations required at that early time, the tanning, the shoemaking, the blacksmithing, the carpentering, the picking, ginning, and packing of cotton, the reaping, threshing, winnowing, and beating of rice, and the machinery required for all these operations, were a constant source of delight and culture to the boys.

It was amid such intellectual and moral influences, amid such country sports and plantation operations, that John Le Conte received his first impressions of life, and under such he grew up until his seventeenth year, when he went to college. This was in 1835.

His early education, received at a neighborhood school, supported by four or five families, was irregular and desultory in the extreme, the teachers, as was common at that time in country schools at the South, changing almost every year. He was fortunate, however, in having as teacher during the last two years, and therefore in immediate preparation for his college course, no less a man than Alexander H. Stephens, afterwards the distinguished lawyer and statesman.

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His collegiate education was received in Franklin College, University of Georgia, located at Athens, where he was graduated with high honor in 1838, and immediately thereafter commenced the study of medicine, the only profession at that time open to a man of scientific tastes and habits. He received his degree of doctor of medicine from the College of Physicians and Surgeons, New York, in 1841. At that time he greatly desired and fully intended to complete his medical education in Paris, but this intention was frustrated by the death of his eldest brother, William, by which it became his duty to administer on the estate of the younger children. His education, therefore, was wholly American. He never crossed the Atlantic. Soon after his graduation in medicine he married Eleanor Josephine Graham, a lady of Scotch descent, with whom he became acquainted in New York during his studies there. Of the three children, the issue of this marriage, only one, L. Julian Le Conte, assistant engineer in charge of Oakland harbor, survives.

Unless we except the early influence of his father, no other influence so greatly affected the whole course of his life as that of his wife. Mrs. John Le Conte was a woman of rare intelligence, spirit, and vivacity and of great force of character, united with queenly beauty and great social influence. He never undertook any enterprise or made any change of life without her advice and counsel. Their mutual devotion was as perfect as human devotion can be, and continued with ever-increasing strength to the very end. If he had lived but two months longer they would have celebrated their golden wedding. He was looking forward to this happy event with eager delight only a few days before his death. The moral effect of such a wedded life who can estimate? Surely it is the most powerful of all influences in ennobling and purifying human character. If character is formed in childhood, it is ripened and refined by a happy wedded life.

As usual in men of science, his life was uneventful in the ordinary sense of the word. His main achievements were in the inner world of thought rather than in the outer world of action. After graduating in medicine he settled in Savannah, and there practiced his profession with moderate success, though still keeping up his pure scientific studies; for it was during this period that he made some very important experiments on the alligator to determine the seat of consciousness and volition in the lower vertebrates. Of the great significance of these experiments we shall speak later.

In 1846, after about four years' practice of his profession in Savannah, he was called to take the chair of physics and chemistry in his Alma Mater, Franklin College, University of Georgia, and there he continued to teach for nine years. He had now at last found his true field of activity and entered upon it with the greatest enthusiasm. As may be anticipated, therefore, he never returned to the practice of medicine, but devoted himself unremittingly to teaching, investigating, and writing on his favorite subjects during the rest of his life—*i. e.*, for forty-five years.

In 1855 he resigned his place in Franklin College to take the chair of chemistry in the College of Physicians and Surgeons, New York, and lectured there on that subject during the winter of 1855-'56; but physics rather than chemistry was his favorite department, and therefore in the summer of 1856 he accepted a call to the chair of physics in the South Carolina College at Columbia. This chair he held until his final move to California, in 1869.

Meanwhile, in 1862, the demands of the terrible war then in progress for soldiers was so severe and sweeping that the college was broken up for want of students. Le Conte was now appointed by the Confederate government superintendent of the extensive nitre works established at Columbia, S. C., with the rank and pay of major, although he never donned the uniform. He retained this place until the end of the war, and during the closing scenes and the march of Sherman through the State he suffered many hardships in the vain attempt to save the property of the Confederate government in his charge. It was during this period of turmoil and anxiety that some of his most important papers were written—papers of pure abstract science—in strange contrast with the furious political storm then raging.

The issue of the war swept clean away all that he owned as property, and the utterly disorganized and prostrate condition of the South left no place for men of scientific tastes and student habits. Therefore, after several years of vain struggle to revive the college and place it again on a firm basis, he was compelled to seek elsewhere for a field of activity. The legislature of California had just then (1868) chartered a State university. It was not yet organized. Some one of experience and reputation was needed for that purpose. He was elected in 1868 to the chair of physics and urged to come at once to assist the regents in the work of organization of the

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university. He went in March, 1869, and the university opened in September of the same year.

During the six months immediately preceding the opening of the new university he was in constant consultation with the regents concerning the organization of its various courses. During the first year after its opening he acted as its president and directed its policy, though continuing still to hold and to perform the duties of the chair of physics. The combined duties of these two positions, however, proving too onerous and his own tastes being stronger in the direction of abstract science, rather than of administrative detail, at the end of the year he withdrew from the presidency to concentrate his energies on the work of his chair. In 1875, on the resignation of President Gilman, he was again asked to act as president, and in 1876 he was elected full president, though still retaining his chair, the duties of which were, however, now lightened by the appointment of an assistant. In 1881 he again and finally resigned the presidency, but retained the physical chair, which he continued to hold up to the time of his death.

Such is a brief history of his connection with the University of California. It is seen that about one-half of his whole life as a teacher was spent in her service. This institution was planted by his hand and grew up under his eye and largely under his guidance. It opened in 1869 with thirty-eight students, eight professors, and an income of about \$30,000. Today it has about twelve hundred students, one hundred and fifty teachers of different grades in all the departments, and an income of about \$350,000. In 1869 it opened as a traditional college of letters and the mere beginnings of a college of agriculture. Today there are some twelve to fourteen colleges, literary, philosophical, and professional. It is everywhere recognized as one of the great universities of our country. There can be no doubt that its remarkable growth and especially its high character for thoroughness is largely the result of his wise course in organization, his wisdom and firmness in guidance, and his wide reputation as a scholar and a man of science. More than any other man he was *the father of the university*. The strong hold which he had taken on the respect, the reverence, and the love of his colleagues, his pupils, the community, and the State, a reverence and love all the deeper for its quietness, was abundantly shown on the occasion of his death. Such an outburst of universal feeling is seldom shown on the death of any man, especially one so retiring

as he. It was the spontaneous homage of a whole people to his character and to his great influence in elevating the whole plane of education in the State.

Scientific Work.

Of his scientific work it is difficult to give any adequate account within the limits of a short memoir; it was so many sided. Although his chief delight was in physics, and in early times, under the influence of his medical studies, also physiology, yet he was not a specialist in the narrow sense in which that term is now used, for, like his father, his mind ranged and his interest extended over the whole realm of nature, though doubtless concentrated on certain departments, especially physics. Thus he fully retained his intellectual perspective—*i. e.*, the perception of the relation of the different departments of nature and the different kinds of truth to one another, so apt to be lost by the mere specialist. His wonderful memory, his methodical manner of reading and recording, and his clearness of physical conceptions gave him a fullness and wideness and yet an accuracy of knowledge rarely attained. Even in my own department of geology, especially when it bordered on physics, I constantly consulted him, with the greatest confidence. In a word, whenever clearness of thought and accuracy of statement on almost any scientific study was required, I instinctively turned to him as I would to a cyclopedia. Verily, the type of physicists to which he belonged can hardly be said to exist any longer. Such men as Newton, Thomas Young, Sir John Hershell, and Wheatstone, in England; as Arago and Laplace, in France, and Benjamin Franklin and Joseph Henry, in this country, were his models. He inherited something of his father's indifference to reputation. At least he had little of the eager desire to rush prematurely into print, too common at the present time. He investigated and pondered long before he wrote, and elaborated his manuscript, both as to matter and as to literary form, with care, often retaining them for years before publication. The amount of his published work, therefore, bore no proportion to the abundance of his knowledge and the wealth of his original thought. Besides his numerous contributions to scientific periodicals, he had commenced during the war and had nearly finished a complete treatise on physics, in which were embodied his wide knowledge and long experience in teaching, but unfortunately this was destroyed during the burning of

Columbia, in February, 1865. On coming to California he commenced again to write it, but the multitudinous details of administrative work left him no time to finish it. A fragment of some hundred pages of manuscript still exists among his papers.

During his long scientific career of just fifty years he published more than one hundred papers, a list of the most important of which are appended. Among these I select for brief analysis a few of value, so great that they have, I believe, distinctly affected the course of scientific progress.

1. *Experiments on the Seat of Volition in the Alligator.* Published in the New York Journal of Medical Science, 1845-'46.

The usual view, up to that time, was (and indeed to some extent still is) that the cerebrum alone is the seat of consciousness and volition, and that the function of the spinal cord is wholly reflex. The object of these experiments was to show, and they did, in fact, very clearly show, undoubted *purposive action* or conduct in the *decapitated alligator* on the application of appropriate stimulation to various parts of the body. The irresistible conclusion was that consciousness and volition are not confined to the cerebrum in the alligator; that the voluntary and reflex functions are not so widely separated and strictly localized in the lower vertebrates as they are in man, but, on the contrary, that these two widely distinct functions overlap and are both widely diffused in the nervous centers of these animals.

Some late physiologists, indeed, would explain these phenomena differently. Recognizing what seems deliberate purposive action in decapitated frogs, they have gone to the other extreme. Instead of extending volition in an obscure form to the spinal cord they would take it away even from the brain. They say that all so-called voluntary purposive action is merely automatic, and even man himself is only a *conscious automaton*. The answer to this is plain. There are, indeed, many conscious automatic actions in man, as, for example, breathing, swallowing, &c.; but there is a wide distinction between these and those we call voluntary.

The true explanation of deliberate purposive action in decapitated reptiles and amphibians is that given in Le Conte's paper and stated above. These experiments were an early illustration of the general law now well recognized that differentiation, specialization, and

localization of functions increase with ascent in the scale of organisms; that functions which are quite distinctly separated and localized in the higher animals are more and more diffused and merged into one another as we go down the scale. Volition and automatism, for example, are not only less distinct as functions in an alligator than they are in man, but they are less localized, the one in the brain, the other in the spinal cord. They are, in fact, widely diffused throughout the nerve centers. It was an early and admirable example of the application of the comparative method in physiology and a proof of its fertility.

2. "*On the Exudation of Ice from the Stems of certain Plants and on the Protrusion of Icy Columns from certain Soils.*" Phil. Mag., 1850.

Curious curling, silky ice-ribbons, looking like bands of fibrous gypsum, exuding from the dead stems of plants near their base are phenomena characteristic of certain annuals common on the coast of Georgia, and therefore familiar to the keen observation of Le Conte from early boyhood. These curling ice-ribbons had been described by others, but had not been explained. The key to the explanation of these he found in the study of the much more widely occurring phenomenon of the protrusion of silky, fibrous transversely striated ice-columns from certain soils, especially and in the greatest perfection from the residual red-clay soils of the Piedmont region of the southern Atlantic States, where they are often five inches in length. The true explanation of both is first given in this paper.

The explanation may be briefly summarized as follows: The necessary conditions are (1) *a firm, yet porous, soil*. This is eminently the case in the residual red-clay soils of the South. (2.) Through this soil water constantly rises and freezes only at the very surface, and thus the ground is kept warm and moist and unfrozen, even in the coldest weather, by the ascending water. If the ground freezes the whole process stops.

Now, imagine a multitude of fine capillary tubes terminating at the surface and water rising and freezing only at the very surface. Each tube would become trumpet-shaped at the mouth by the expansion of the water in the act of freezing and the consequent condensation of the clay between. Freezing and expansion in this trumpet-shaped mouth would produce a sudden infinitesimal jump

of the ice upward. This in its turn would draw up the water from below and again fill the trumpet-shaped mouth, and the same operation would be repeated indefinitely. The capillary fibers cohering give rise to the silky, fibrous columns; the paroxysmal upward jumping gives rise to the transverse ridging of each fiber and thus to the transverse striation of the columns. The phenomenon is so curious and so beautiful that it attracts attention everywhere, and it is remarkable that no explanation had before been attempted.

The explanation of the curling, silky ice-ribbons exuding from the stems of plants is precisely the same. Here the wood pores take the place of the earth pores. Here, also, if the stem freezes the process stops. The flat ribbon shape is given by the protrusion of the ice through fissures in the bark.

3. "*The Influence of Musical Sound on the Flame of a Gas-jet.*" Am. Jour. and Phil. Mag., 1858.

This was the first notice and explanation of the beautiful phenomenon of sensitive flames now so familiar to physicists. The explanation is so well known that it need not be introduced here.

The importance of this discovery cannot be overestimated, for it was not only the discovery of a new and beautiful phenomenon, but it introduced a new method of research, which, in the hands of Barrett, Tyndall, Koenig, and others, has revolutionized the science of acoustics. By means of this marvelously delicate test of gaseous vibrations refraction and diffraction and interference of sound waves are easily demonstrated. Complex sound waves are analyzed and their components rendered visible. The analogy of sound and light is thus made clear and the wave theory of light itself placed on a surer basis.

4. *The Adequacy of Laplace's Explanation to account for the Discrepancy between the Computed and Observed Velocity of Sound in Air and Gases.* Philosophical Magazine, January, 1864.

This paper was written in 1861, during the war between the States, at a time, therefore, when the author was cut off by the blockade from communication with the scientific world. It was held in the hope of an opportunity of publication, which, however, did not come until 1864.

There is a slight discrepancy between the observed and calculated velocity of sound in air and gases, the former being slightly in excess. Laplace explained this by the increased elasticity due to the heat generated by compression at the wave crest. This increased elasticity by heat of condensation is not compensated for by any opposite effect of the cold of rarefaction at the wave trough, as might at first be supposed. On the contrary, the two causes co-operate, and their effects must be added, the propagating force, so far as this cause is concerned, being the difference of elasticities of crest and trough. Many physicists held that this explanation was inadequate; that the cause was not sufficient to produce the effect, and therefore that the whole theory of the propagation of sound waves needed complete revision. Several papers had about this time appeared in the *Philosophical Magazine*, especially one by Earnshaw, to prove this inadequacy. This paper of Le Conte aimed to show, by calculations based on the latest and most reliable experimental data of Regnault and others, the complete adequacy of Laplace's explanations. The paper showed complete mastery of the whole subject, both as to the physical principles and to the previous literature, and received the warmest commendations of Sir William Thomson (now Lord Kelvin), Tyndall, and others eminent in physics. It called out several replies, but it is believed that the conclusions will stand the test of time.

In July, 1876, being one of a party of men of science specially invited to examine the first telephones, then on exhibition at the World's Fair at Philadelphia, I there met Sir William Thomson. On being introduced he immediately asked me if I was the author of "that remarkable paper on sound," of which he again expressed his warm admiration. On replying in the negative I could not but observe a shade of disappointment flit across his face.

5. "*Sound Shadows in Water.*" *American Journal*, 1881, and *Philosophical Magazine*, 1882.

This was an account and a discussion of results obtained during an experimental investigation carried on at his suggestion by his son, L. Julian Le Conte, assistant engineer, in charge of improvements in the harbor of San Francisco. Rincon rock, a sunken ledge obstructing the landing along a portion of the water front, was being removed by blasting with dynamite. This afforded an excellent

opportunity of experimentally verifying the theoretical cause of the difference between sound shadows and light shadows.

Light shadows are sharply defined geometric projections of the object. Sound shadows, on the contrary, are so blurred on their edges that their limits cannot usually be determined with certainty. The reason of the difference is the extreme shortness of the waves in the one case and their great comparative length in the other. The character of the shadows in the two cases has been accurately calculated, but experimental verification for sound waves of different lengths was still wanting, although it was known in a general way that the shadows of acute sounds were sharper than those of grave sounds. Dr. Le Conte's experiments were on shadows made by subaqueous explosions of nitro-glycerine. The sound waves made by such explosions are admirably adapted for the purpose for two reasons. First, because they were subaqueous. Water carries short sound waves better and farther than long waves and better than air. Colladon's experiments on Lake Geneva had already shown that of the many waves of different lengths made by striking a bell under the water only the shortest were carried to any considerable distance. The sharp click of the hammer was heard much farther than the musical sound. A second reason is because the extreme suddenness of nitro-glycerine explosion generates waves of almost inconceivable shortness. The shadows of such waves ought by theory to be exceptionally sharp, and such by experiment was found to be a fact. Stout glass bottles exposed to the direct action of these waves at a distance of fifty or more feet were invariably shattered, while behind an obstacle, such as a pier or pile, even at a considerable distance behind it and near the edge of the geometric shadow, they were completely protected. Thick glass rods behind an obstacle one foot in diameter and twelve feet away from it were sharply broken on each side at the margin of the geometric shadow, the length of the unbroken part being exactly equal to the diameter of the obstacle.

6. *Physical Studies of Lake Tuhoe*. Overland Monthly, 1883 and 1884.

Although published in a literary periodical and therefore written in somewhat popular style, this is really a scientific paper of great importance. It is, in fact, a perfect model of what a popular scientific article ought to be, for it is simple in style and yet thoroughly

scientific in matter ; it is perfectly intelligible to the educated public and yet of deepest interest to the expert physicist as a real contribution to science. The paper is an exhaustive discussion of all the most interesting questions connected with mountain lakes in general, and this one in particular as the finest of all examples of such lakes. Among the questions discussed are depth, distribution of temperature, color, and rhythmic oscillations of level or *sèche*, &c. ; but its chief value consists in its admirable discussion of the blue and green color of pure water and the blue color of the sky. We will therefore confine our analysis to this point alone.

He had long been intensely interested in the beautiful investigations of Tyndall on the blue color of the sky and of Soret on the blue and green color of the waters of Lake Geneva, but the explanation was still unsatisfactory ; further investigations were needed. He had already himself, in 1860, published some observations on the marvelous transparency of the water of Silver spring, in Florida, probably the most transparent water in the world. (Proc. A. A. S., 1860 ; Am. Jour., 1861.) But the water of this wonderful pool was not deep enough (about forty feet) to bring out color. Except, perhaps, the Mediterranean, there is no water so well adapted for this purpose as that of Lake Tahoe. Its depth (1,645 feet, as determined by Le Conte) is far greater, its water far purer (the limit of visibility about double), and therefore the splendor of its blue color far finer than that of Lake Geneva.

Dr. Le Conte's observations on Lake Tahoe were made in 1873 while spending his summer vacation on the lake, but the investigations were continued in his laboratory by experiments on the *effect* of transmission of light through long tubes filled with distilled and with natural waters respectively on the color of the emergent beam. The final results were not published until 1883, although reached and embodied at least five years earlier. In the meantime Soret had been carrying on similar investigations on Lake Geneva. These were published partly in 1878, but mainly in 1884. The results of the two investigations were nearly identical, although wholly independent of each other.

Le Conte's paper was an exhaustive discussion of the whole subject of colored media of all kinds and the cause of color in each case. The general result of the experimentation and subsequent discussion was that the blue color of water is due partly to *selective absorption*—greater for the *red* end than the blue end of the spec-

trum—with diffuse *molecular reflection* of the unabsorbed blue rays, and partly to *selective reflection* from suspended particles, which, if small enough, reflects mainly the blue rays. In absolutely pure distilled water only the first cause operates and water is blue as blue glass is blue; in natural water, on the contrary, the second is the main cause.

This view, arrived at independently by Soret and Le Conte, completely explains all the phenomena of the color of mountain lakes, of the ocean, and of the sky. For example:

Blue Color.—It follows from the above that if the water is pure enough, the suspended particles are small enough, and the lake is deep enough so that *all* the light that comes to the eye is from internal reflection, *the color will be blue*, and the splendor of the blue will be in proportion to the purity of the water and the smallness of the suspended particles.

Green Color.—The green colors of natural waters are of such various shades, depending on admixture of sediments, color of bottom, and nature of dissolved organic matter, that each case must be investigated for itself. I shall speak, therefore, only of the green color of the purest mountain lakes.

Let it be remembered, then, that of the two causes of blue color mentioned above the first—*i. e., selective absorption*—would give blue, both by reflected and by transmitted light; but the second or main cause—*i. e., selective reflection*—would by itself produce more or less completely complimentary yellow or orange by transmitted light; but as the two causes are always combined in natural waters, the real color of transmitted light in the case of purest natural water was found by Le Conte's experiments to be yellowish orange to yellowish green.

Let it be remembered again that the green color of purest and deepest mountain lakes is found only on the margin, where the water is shallow, but not too shallow, and that it is especially splendid when the bottom is white sand, and therefore a good reflector. Now, suppose we have a moderate depth of twenty to thirty feet and a white bottom, the light reflected from suspended particles, as already seen, would be blue, while the light going through, striking the bottom and reflected back to the eye, would be yellow or greenish yellow. The combination of this with the blue of selective reflection would make various shades of green, according to the depth of the water.

This paper, although characterized by Soret as "un beau memoir," yet, on account of its having been published in a literary periodical, was not as widely known among scientific men as it deserved. It is for this reason I have given a fuller analysis.

Many other papers might be mentioned as almost equally deserving of attention, but space and time forbid. Among the last of his investigations were those on *capillarity*, a subject admittedly one of the most obscure and refined in physics, requiring both clearness of physical conceptions and mathematical skill, and for that very reason having a great charm for him. Thus he continued, though with slowly decreasing energy, to work and perform every duty up to a few days of his death. As he walked to and from his daily duties his slender figure, bowed form, and abundant snowy hair and beard, like an aureole about his noble head and benignant face, will long remain in the memory of his friends as the outward visible expression of one of the noblest and purest of human spirits.

In June, 1889, his strength visibly failing, he was given a year's leave of absence for travel, recreation, and sorely needed rest. Unfortunately, on the eve of his intended departure for Europe, where he expected to visit personally his many correspondents, his wife was prostrated with protracted illness. For nearly a year he scarcely left her bedside, and only his tender, unremitting care nursed her back to life and comparative health. The opportunity of recreation was lost. Unrefreshed at the beginning of the session, August, 1890, he took up again the burden of duties, and would have been able to bear it to the end of the session, when it was arranged he would be permanently relieved, but an attack of la grippe easily overcome his weakened frame, and after an illness of a few days he died, April 29, 1891.

Character.

It does not, perhaps, become me, who stood so near to him, to speak at any length of his character; but one trait I cannot pass over, for it was bound up with his whole intellectual and moral nature. His simple love and earnest seeking of objective truth had, as it were, burned in through the intellectual and into the moral nature of the man, intensifying his inherited love of truthfulness. His life-long habit of implicit reliance on unchanging natural law served to increase to a passionate degree his native integrity. Truth

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he loved and even worshiped as the image of God in the human reason, and truthfulness he honored as the very basis of human character. Therefore next to his devotedness in nearest human relations, and indeed closely bound up with this, was his absolute transparent truthfulness and his utter scorn of the least approach to falseness in word or deed or any indirectness in methods of attaining a moral end. His first public address in an official capacity as president, in Oakland, and also his last, in Berkeley, was on this subject.

But I dare not speak further on the subject of his innermost character. Perhaps my relations with him were too close, my view of him too near, to see well the general effect. The beating heart may make unsteady the hand that would draw a true picture. Let those speak, therefore, who loved him and yet stood a little farther away. I cannot do better than quote from a memorial drawn up by a committee of his colleagues on the occasion of his death.

"Admirable as were the scientific powers of our associate and distinguished as were their results, it was even more in the general temper and spirit of his mind and life that we found his chief attraction. He was, indeed, wholly and purely of scientific spirit and scientific habit. He literally lived and breathed in an atmosphere of scientific thought, and yet, as much as this disinterested scientific spirit attracted our attention and won our admiration, it was rather the moral traits of our friend that constituted his eminent worth and drew forth our warmest admiration and love. Seldom has a man exhibited a more unvarying and entire disinterestedness. We can truly say that he seemed to us utterly destitute of what are called ulterior motives. He loved truth and truthfulness supremely. Fealty to all truth, whether scientific or philosophical, theological or æsthetic, and fealty to all persons were not so much qualities of him as sum and substance of the man. He was simplicity and ingenuousness embodied. He was sympathetic, generous, tender, brave, and the soul of honor. It is not for us to invade the sacred precincts of his more intimate relations of home and kindred, but what he was there was evident enough, even to those who saw him from afar."

And, again, one of his colleagues who has known him longest, Martin Kellogg, now president of the university, writing of him in the *Overland Monthly*, says :

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"And so we commend our loved colleague to our young men for the scholarly breadth of his culture, as well as for its completeness in his own department of science.

"But he had yet stronger hold on the admiration of his friends and the veneration of his pupils. It is the nobleness of his character. Scholar and scientist might be less important to some, but no one could fail to recognize in him the Christian gentleman and nobleman. This gave him commanding influence in the community, in the State, and in all the States in which he had lived. Among all the men connected with our educational interests no one could have a larger circle of sincere mourners. He was so gentle and genial, so learned in science, so highly reputed for his work and its published results, so self-poised in his judgments, so catholic in his recognition of all higher interests, so honored in the esteem of his colleagues, so beloved by many successive generations of students, that a great void is left by his departure. His memory receives such tributes of praise as are given to few men in all our wide land—eulogies won by a long life of beneficent activity and of rare purity and loftiness of character."

The Le Conte Memorial Fellowship, with endowment of \$10,000, established by the alumni of the university soon after his death, and a bronze bust of him as he appeared draped in presidential robes, given also by the alumni and now in the library of the university, are testimonials of the love and veneration in which his memory is held by them.

In looking over what I have said of his character I perceive that it is incomplete; that one side is wholly unrepresented. The cause of this is plain. In my own case the sense of irreparable personal loss and in the case of his colleagues the recency of his death at the time they wrote has tinged the picture with a too somber hue. The effect of the whole, therefore, may seem sad. If so, this is a reflection of the feelings of his friends, not of his own nature. Next to his devotedness to persons and to truth his most pronounced characteristic was his warm, genial, sunny temperament. This was conspicuous on all occasions and to every one, but especially to visitors under his own roof. Those who saw him there felt at once instinctively the hearty hospitality of an ideal home. They will remember his kindly cheerfulness, his playful humor, and his con-

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tagious laughter; in a word, his full enjoyment of happiness in conferring happiness. He doubly enjoyed every pleasure in sharing it with a friend. Indeed, such enjoyment of life was but the necessary complement of his truthfulness, his genuineness, his heart-soundness, his unostentatious righteousness.

His colleagues have loved to hold him up to themselves as an example to imitate and to the young men as an ideal to follow. May we not also hold him up to men of science, especially in these days of extreme and sometimes narrow specialization, in these days of profitable science and often of science for profit? May we not hold him up to scientific men of our country as the embodiment of the broadest, the truest, the most disinterested spirit of science?

A PARTIAL LIST OF HIS SCIENTIFIC PAPERS.

1. "Case of Carcinoma of the Stomach." ("New York Medical Gazette," 1842.)
2. "On the Mechanism of Vomiting." ("New York Lancet," 1842.)
3. "On Carcinoma in General and Cancer of the Stomach." (*Ibid.*, 1842.)
4. "On the Explanation of the Difference in Size of the Male and Female Urinary Bladder." (*Ibid.*, 1842.)
5. "An Essay on the Origin of Syphilis." ("New York Journal of Medical and Collateral Sciences," 1844.)
6. "Remarks on Cases of Inflamed Knee-joint." (*Ibid.*, 1844.)
7. "Extraordinary Effects of a Stroke of Lightning, Singular Phenomena." (*Ibid.*, 1844.)
8. "Observations on Geophagy." ("Southern Medical and Surgical Journal," 1845.)
9. "Experiments Illustrating the Seat of Volition in the Alligator, or *Crocodilus Lucius* of Cuvier, with Strictures on the Reflex Theory." ("New York Journal of Medical and Collateral Sciences," 1845 and 1846.)
10. "Statistical Researches on Cancer." ("Southern Medical and Surgical Journal," 1846.)
11. "On the Quarantine Regulations at Savannah, Ga." ("New York Journal of Medical and Collateral Sciences," 1846.)
12. "Remarks on the Physiology of the Voice." ("Southern Medical and Surgical Journal," 1846.)
13. "Dr. Bennet Dowler's Contributions to the Natural History of the Alligator." (*Ibid.*, 1847.)
14. "On Sulphuric Ether." (*Ibid.*, 1847.)
15. "The Philosophy of Medicine, an Address." (*Ibid.*, 1849.)
16. "Observations on a Remarkable Exudation of Ice from the Stems of Vegetables, and on a Singular Protrusion of Icy Columns from certain kinds of Earth during Frosty Weather." ("Proceedings of the American Association for the Advancement of Science," 1850; "Philosophical Magazine," 1850.)
17. "Observations on the Freezing of Vegetables and on the Causes which enable some Plants to endure the Action of Extreme Cold." ("American Journal of Science," 1852; also "Proceedings of the American Association for the Advancement of Science," 1853.)
18. "On the Venomous Serpents of Georgia." ("Southern Medical and Surgical Journal," 1853.)

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19. "On the Descent of Glaciers." ("American Journal of Science," 1855.)
20. "Review of Lieutenant M. F. Maury's Work on the 'Physical Geography of the Sea.'" ("Southern Quarterly Review," 1856.)
21. "The Mechanical Agencies of Heat." (*Ibid.*, 1856.)
22. "Influence of the Study of the Physical Sciences on the Imaginative Faculties." An Inaugural Address, delivered December 1, 1857 (Columbia, S. C., 1858).
23. "Preliminary Researches on the alleged Influence of Solar Light on the Process of Combustion." ("American Journal of Science," 1857; also "Proceedings of the American Association for the Advancement of Science," 1857, and "Philosophical Magazine," 1858.)
24. "On the Influence of Musical Sounds on the Flame of a Jet of Coal Gas." ("American Journal of Science," 1858; "Philosophical Magazine," 1858.)
25. "On the Optical Phenomena presented by the Silver Spring in Marion County, Florida (U. S.);" ("American Journal of Science," 1861; also "Proceedings of the American Association for the Advancement of Science," 1860).
26. "On the Adequacy of Laplace's Explanation to Account for the Discrepancy between Computed and the Observed Velocity of Sound in Air and Gases." ("Philosophical Magazine," 1864.)
27. "Limiting Velocity of Meteoric Stones reaching the Surface of the Earth." ("Nature," 1871.)
28. "Vital Statistics, Illustrated by the Laws of Mortality from Cancer." ("Western Lancet," 1872.)
29. "Heat Generated by Meteoric Stones in Traversing the Atmosphere." ("Nature," 1872.)
30. "The Nebular Hypothesis." ("Popular Science Monthly," 1873.)
31. Articles on "Bonanza," "Comstock Lode," and "Death Valley," in Johnson's Cyclopaedia, vol. iv, appendix, 1876.
32. "Mars and His Moons." ("Popular Science Monthly," 1879.)
33. "Origin and Distribution of Lakes; Meteorology of the Pacific Coast." ("Mining and Scientific Press" and Supplement, 1880-'81.)
34. "Influence of Modern Methods of Popularizing Science." ("Berkeleyan," 1882.)
35. "Sound Shadows in Water." ("American Journal of Science," 1882; also "Philosophical Magazine," 1882.)
36. "Origin of Jointed Structures in Undisturbed Clay and Marl Deposits." "American Journal of Science," 1882.)
37. "Apparent Attractions and Repulsions of Small Floating Bodies." ("American Journal of Science," 1882; also "Philosophical Magazine," 1882.)

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38. "Amount of Carbon Dioxide in the Atmosphere." ("Philosophical Magazine," 1882.)

39. "Physical Studies of Lake Tahoe." ("Overland Monthly," three papers, 1883, 1884.)

40. "The Part Played by Accident in Discoveries." ("Berkeleyan," 1884.)

41. "Horizontal Motions of Small Floating Bodies in Relation to the Validity and Postulates of the Theory of Capillarity." ("American Journal of Science," 1884; also "Journal de Physique," 1885.)

42. "Criticism of Bassnett's Theory of the Sun." ("Overland Monthly," 1885.)

43. "The Evidence of the Senses." ("North American Review," 1885.)

44. "The Metric System." ("Overland Monthly," 1885.)

45. "Thought Transference." (*Ibid.*, 1885.)

46. "Barometer Exposure." ("Science," 1886.)

47. "Electrical Phenomena on a Mountain." (*Ibid.*, 1887.)

48. "Standing Tiptoe, a Mechanical Problem." (*Ibid.*, 1887.)

49. "Vital Statistics and the True Coefficient of Mortality, illustrated by Cancer." ("Tenth Biennial Report of the State Board of Health of California," 1888.)

50. "The Decadence of Truthfulness." (1889.)

SOME MINOR CONTRIBUTIONS.

51. "On a Topographical Survey of the State of South Carolina." ("Lieber's Third Annual Report on Geological Survey of South Carolina," 1859.)

52. "Table of Physical Constants." ("Smithsonian Report," 1878.)

53. "Limiting Velocity of Meteoric Stones reaching the Earth." ("Nature," vol. iv, 1878.)

54. "Expansion of Glass by Heat." ("Nature," 1880.)

55. "Ice Crystals." ("Nature," 1880.)

56. "Solid Ice at High Temperatures." ("Nature," 1880.)

57. "On the Space Protected by Lightning Conductors." ("Nature," 1881.)

58. "The True Coefficient of Mortality." ("Nature," 1881.)

59. "Photography of Diffraction Rings." ("Nature," 1881.)

60. "Apparent Attraction of Small Floating Bodies." ("Science," 1883.)

61. "Thermal Belts in North Carolina." ("Science," 1883.)

62. "Hydrogen Whistles." Showing the error of Galton. ("Nature," 1883.)

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63. "Sun's Radiation and Geological Time." ("Science," 1883.)
64. "Solar Constants." ("Science," 1883.)
65. "Upper Glow of the Skies in Relation to Halos and Coronæ." ("Science," 1884.)
66. "Remarkable Sunsets." ("Nature," 1884.)
67. "Velocity of Atmospheric Waves from Krakatoa." ("Science," 1884.)
68. "Points on Lightning Rods." ("Science," 1884.)
69. "Do Young Snakes Take Refuge in the Stomach of the Mother?" ("Nature," 1886.)
70. "Flooding of Sahara." ("Science," 1886.)
71. "Deepest Fresh-water Lake in America." ("Science," 1886.)
72. "Lightning Flashes: Their Direction Undeterminable by the Eye." ("Nature," 1887.)
73. "Noctiluca Clouds." ("Nature," 1889.)
74. "Relation of the High Schools to the University." ("Berkeleyan," 1877.)
75. "Importance of Unity in the Methods of Instruction in the Public Schools." ("California Teacher," 1885.)
76. "Igneous Meteors." ("Mining and Scientific Press." Supplement, 1879.)
77. "Qualifications of Teachers in the Primary Schools." ("Pacific School and Home Journal," 1887.)
78. "The Part Played by Accident in Discoveries in Science." ("Berkeleyan," 1884.)
79. "Review of Bassnett's Theory of the Sun." ("Overland Monthly," 1885.)
80. "Review of Arthur Kimball's Physical Properties of Gases." ("Overland Monthly," 1890.)
81. "Review of Robert Thurston's Heat as a Form of Energy." ("Overland Monthly," 1891.) This last was published a few days after his death.



J. V. Hayden

MEMOIR
OF
FERDINAND VANDIVEER HAYDEN.
1839-1887.

BY
CHARLES A. WHITE.

READ BEFORE THE NATIONAL ACADEMY, NOVEMBER, 1894.

BIOGRAPHICAL MEMOIR OF FERDINAND V. HAYDEN.

Mr. President and Members of the Academy:

Although Dr. Hayden was so long and so widely known to the public, and although I was many years personally acquainted with him, I have met with considerable difficulty in obtaining certain data upon which I desired to base the memoir which I have prepared, and some which I thought it would be appropriate to use I have been quite unable to obtain. The memoir is therefore not so full in some respects as I wished to make it. A part of the statements therein contained are the result of my personal recollections, but most of the facts which relate to his official work have been collated from public records. Those which relate to his personal history I have obtained mainly from his widow, Mrs. Emma W. Hayden, but in large part from Dr. Albert C. Peale, who was his intimate friend to the end of his life and who has himself prepared a biographical sketch of Dr. Hayden. The manuscript of that sketch Dr. Peale has generously allowed me to consult in the preparation of this memoir.

Ferdinand Vandiveer Hayden was born at Westfield, Massachusetts, September 7th, 1829. He was a son of Asa Hayden, whose father is said to have served in the Continental army during the Revolutionary war. Ferdinand's father died when the former was about ten years old, and his mother, marrying again, went to live in New York State. Two years after his father's death the boy, finding his home uncongenial, went to live with his uncle on a farm near Rochester, Lorain county, Ohio.

I have been able to learn comparatively little concerning Ferdinand's boyhood, but it is evident that he was unusually studious, and that he fully improved all the opportunities he had for study, because, notwithstanding his exacting labors on the farm, he was able to teach successfully in the district school of his neighborhood during the winter of his sixteenth year. He continued to work on his uncle's farm and to teach during the winter months until he was eighteen years of age. His uncle then offered to adopt him as his

son, but Ferdinand promptly declined the offer, the life of a farmer being uncongenial to his tastes and not in accord with his ambition. He was eagerly desirous of an education and, leaving his uncle's house, he went to Oberlin, the seat of Oberlin College, and being without money to pay for his transportation, he walked all the way. Arriving in Oberlin, he stated his case and his desires to President Finney. That earnest and experienced educator extended to the anxious boy his advice and sympathy, the result of which was that he began at once the requisite preparation for a college course at Oberlin, working meanwhile to earn money to pay for his support and tuition.

Some of those who were his classmates have spoken of him as having been shy and diffident, and despite his acknowledged earnestness and studious habits, they did not predict for him a prominent career; but it may well be questioned whether that shyness and diffidence were inborn or due to severe, if not cruel, buffetings which the well-nigh friendless boy encountered almost from his infancy.

Having completed his preparation in 1847 he entered the class of 1850, overtook them in the advance they had made in the course of study before he entered, and graduated with them in the last-named year. It is a remarkable but apparently a well authenticated fact that from the time he left his uncle's house almost penniless to the time of his graduation he never received any pecuniary assistance, but paid all his expenses with the labor of his own hands, and meantime more than kept pace with the studies of his class.

Upon his graduation from Oberlin with the degree of A. M. he devoted all his time to the study of medicine, having chosen that profession for his career. In pursuance of this object he at once matriculated at Albany Medical College, Albany, New York, and was graduated from that institution with the degree of Doctor of Medicine in 1853.

During his course of medical study at Albany he became acquainted with Prof. James Hall, and through that acquaintance became intensely interested in the subject of geology. At this time public attention was largely drawn to the discoveries of fossils which had been made west of the Mississippi by Dr. John Evans, Dr. D. D. Owen, and others. Professor Hall was desirous of obtaining information concerning the geology of that region, toward which he had already extended his own observations. Finding his young medical

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acquaintance enthusiastic, that experienced geologist proposed to furnish the means for exploration, provided he should have the results for study and publication. Accompanied by Mr. F. B. Meek, who was then acting in the capacity of paleontological artist to Professor Hall, Dr. Hayden left Albany in the spring of 1853 for what was then known as the Upper Missouri Bad Lands. The two young men visited the region where five or six years before Dr. John Evans had made his wonderful discoveries of fossil vertebrate remains. Returning to Albany in the autumn of the same year, Dr. Hayden faithfully fulfilled his agreement with Professor Hall, delivering up to him all his collections and field notes, most of which pertained to formations of Cretaceous and Tertiary age.

The vertebrate remains of those collections were studied by Dr. Joseph Leidy, and the Cretaceous invertebrates were published jointly by Professor Hall and Mr. Meek in the *Memoirs of the American Academy of Arts and Sciences at Boston*. That publication was accompanied by a vertical section of the formations from which the fossils were obtained, which is understood to have been the joint work of Dr. Hayden and Mr. Meek, although the name of the former does not appear in the publication referred to.

During his exploration for Professor Hall, Dr. Hayden became so thoroughly interested in the great region then generally designated as Nebraska, but now comprising several large States, that he determined to devote himself to its geographical and geological exploration. His labors in that direction for the next twenty-five years, and the manner in which he prosecuted them, are so characteristic of the man that a concise and consecutive account of them, followed by a statement of other matters of his personal history, will, I think, best illustrate my subject.

Not having money enough to carry on independently the explorations upon which he had determined, he sought and obtained permission to travel a part of the time with parties of the American Fur Company on their annual trips for trade and hunting in that country. Starting in the spring of 1854, two years were spent by him in toilsome journeys, traveling largely on foot, often sleeping upon the ground where night overtook him, and occasionally eking out his expenses by working for the few traders who were scattered over that region. In this way he traversed the valley of the Missouri river from the western boundary of the then Territory of Iowa to Fort Benton, now a principal town of the state of Montana.

He also traversed the valley of Yellowstone river from its mouth to the mouth of Big Horn river. In these journeys his sources of supply and protection were the boats of the American Fur Company, which were then ascending those rivers. Their slow progress enabled him to spend much of his time ashore, noting geological structure and collecting fossils. Returning from these long, wild journeyings, Dr. Hayden reached Saint Louis, Missouri, early in 1856, and for a time made his headquarters there.

The geological notes made by Dr. Hayden during those two years of exploration have served as the basis for constructing a section of the formations of that region which remains with only slight changes to this day, and his geological notes served in part as the basis of subsequent explorations under the auspices of the United States War Department. The large collections of fossils which he then made opened to the scientific world a knowledge of extinct faunas such as had never before been known, and the discoveries which he thus inaugurated have been among the most remarkable in the history of biological geology.

Dr. Hayden deposited his large collections of fossils in part with the Academy of Science of Saint Louis, and in part with the Academy of Natural Sciences of Philadelphia. The vertebrate remains were described by Dr. Joseph Leidy, and the invertebrate remains were published jointly by Mr. Meek and Dr. Hayden, the greater part of all these descriptions having first appeared in the publications of the Philadelphia Academy.

A few weeks after Dr. Hayden's return to Saint Louis he received from Lieutenant (afterward General) G. K. Warren, United States Topographical Engineers, a proposition to make a report upon the geographical and topographical features of the region which he had traversed during the three preceding years. The knowledge thus obtained by Dr. Hayden was desired by the United States War Department that it might be made available by a party in charge of Lieutenant Warren, which the department was about to send out to the region then generally spoken of in connection with military operations as the Sioux country. Dr. Hayden at once accepted this proposition and set about preparing the report. Meantime Lieutenant Warren was preparing for his departure, and, having appointed Dr. Hayden as an assistant, they set out in the following May for "the exploration of the Yellowstone river from Fort Pierre to a point 60 miles north of the mouth of the Yellowstone."

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Having accomplished its purpose the party returned, reaching Washington in November, 1856.

In the spring of 1857 Lieutenant Warren organized a party for exploring the region about the Black hills, and in May of that year Dr. Hayden was appointed as its geologist. The party started from Sioux City, Iowa, and proceeded westward to Loup fork of Platte river, following up its valley to the region of their special work. The return was made late in the same year, Dr. Hayden reaching Washington in December.

The summer of 1858 was spent by Dr. Hayden and Mr. Meek in company, exploring various parts of the then Territory of Kansas. This exploration was very successful, especially in the collection of fossils, which were published under the names of Meek & Hayden, a joint authority which from that time forward became familiar to all readers of geological literature.

Early in 1859 Captain (afterward General) W. F. Reynolds, of the United States Topographical Engineers, was instructed to "organize an expedition for the exploration of the country through which flow the principal tributaries of the Yellowstone river, and of the mountains in which they and the Gallatin and Madison forks of the Missouri have their source." Dr. Hayden was appointed surgeon and naturalist of that expedition in April of that year, and leaving Saint Louis on May 28 the party proceeded up the Missouri river to Fort Pierre. From Fort Pierre they journeyed westward, and a part of the objects of the expedition was accomplished during that season. At the close of the season the party turned southward and went into winter quarters near the upper portion of the valley of the North Platte. Explorations were resumed in May of the following year, and in October the party reached Omaha on their return, where it was disbanded, and Dr. Hayden returned to Washington.

The outbreak of the civil war put an end to all western exploration and Dr. Hayden entered the Union Army. His first appointment was acting assistant surgeon, but he gradually rose to post surgeon, surgeon-in-chief of division of cavalry, and at the close of the war he was brevetted lieutenant colonel for meritorious services during the war.

In 1865 Dr. Hayden was elected Professor of Geology and Mineralogy in the University of Pennsylvania, and held that position for seven years, but an arrangement was made by which his duties

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at the university were not to interfere with western explorations, his plans for the prosecution of which he had never abandoned.

In 1866 he made a second trip to the Bad Lands of Dakota, as they were then called, the later one being under the auspices of the Academy of Natural Sciences of Philadelphia. On this trip he reviewed many points exhibiting geological structure which had been previously visited by him, made notes of the same, and brought back large and important collections of fossil remains.

Upon the admission of Nebraska as a State, in 1867, Congress set apart five thousand dollars, an unexpended balance of appropriation for legislative expenses. This sum was devoted to a geological survey of the new State, and Dr. Hayden was placed in charge. In 1868 five thousand dollars more was appropriated, and he was authorized to extend his work westward into Wyoming Territory.

For the results of the explorations of these two years Dr. Hayden made his reports to the Commissioner of the General Land Office, but in 1869, Congress having made an appropriation for continuing the work in the territories, it was placed under the Secretary of the Interior, and Dr. Hayden was continued in charge. It was thus that what was afterward known as the United States Geological Survey of the Territories was begun.

The work for 1869 consisted of a reconnoissance by the whole party of ten persons along the eastern front of the Rocky mountains from Cheyenne to Santa Fé, and journeys by Dr. Hayden into Middle Park and Great Salt Lake basin.

The expedition of 1870 was organized in a similar manner to that of 1869, and the areas reconnoitered were a belt of country along the eastern base of the Laramie range, the upper valleys of the North Platte and Sweetwater rivers, besides portions of the Laramie plains, of the northern slope of the Uinta range, and of Green river basin.

The parties of the expedition of 1871 consisted in all of about thirty persons, including laborers. The organization of the expedition was similar to that of the previous year, except that two topographers were employed, but the work was mainly a reconnoissance, as that of all the previous expeditions had been. A portion of the region about the sources of Yellowstone river was explored, the outfit for the field having been made from Ogden, Utah, which was made practicable by the then lately constructed Union Pacific railway. The results of this season's exploration attracted the widest

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interest, and Congress readily complied with Dr. Hayden's recommendation that the region he had last explored be set apart as a National Park. This plan was an original conception of Dr. Hayden and one for which he deserves unstinted credit.

The work of 1872 was far more systematic than that of any of the preceding years, a true geographical and topographical feature having been added to it. Two divisions were organized for field work, the first directing its labors mainly to the region which afterward became the National Park, and the second explored the region drained by Snake river, the southern great tributary of Columbia river. This party made important corrections in previously published maps.

In 1873 the work assumed a still more systematic form, and more men of scientific ability were added to the corps. Colorado was the field of operations, and three divisions were organized, one operating in Middle Park, one in South Park, and the other in San Luis Park.

The work of the survey for the years 1875 and 1876 was confined mainly to Colorado, its organization having been similar to that of the two preceding years. A large part of the work of 1877 was also done in Colorado, but two of the five field parties operated in a region comprising portions of Wyoming, Utah, and Idaho.

In 1878, appropriations having been reduced, parties of the survey operated only in the Yellowstone Park, Snake river and Wind river regions. This was the last year of the work organized by Dr. Hayden, it having been superceded by the organization of the present United States Geological Survey in the following summer.

While the explorations and surveys under Dr. Hayden were in progress other persons from time to time obtained appropriations from Congress for similar organizations, all of which operated independently of one another. The result was that a rivalry arose between them which was believed to be detrimental to science and the interests of the Government; consequently Congress determined to bring them all under one organization, and in 1879 all of them, including the one under Dr. Hayden, were disbanded and the present United States Geological Survey was organized.

In view of the extent, long duration, and success of Dr. Hayden's labors, he felt, as did a large proportion of his friends, that he ought to have been placed in charge of the new organization, and he and they were disposed to regard his retirement as an unjust reflection

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upon him. He was then in the full possession of his physical and mental vigor and felt very keenly his forced retirement from the executive prosecution of a work which he had established with so much labor and hardship and the expenditure of the best energies of his life.

When, however, the new organization was accomplished and another man placed in charge of it, he submitted without complaint and accepted a subordinate position upon the survey for the purpose of enabling him to complete certain portions of the work which he had begun. He retired to Philadelphia, where during the following four years he occupied himself with the work mentioned, making occasional journeys to the scenes of his early labors in the west.

Not long after his retirement it became apparent to his friends that his health was not so vigorous as formerly, but it was not until 1882 that alarming symptoms of the disease of which he died were manifested. His disease, locomotor ataxia, made its usual progress and at the end of the year 1886 he voluntarily severed his connection with the United States Geological Survey, thus ending nearly thirty years of honorable service to the Government as naturalist, geologist and surgeon.

An account of Dr. Hayden's official career is practically the story of his life during that time, and I am able to add comparatively little to the foregoing narrative besides some references to his domestic life and his personal characteristics.

On November 9, 1871, he was married to Miss Emma C. Woodruff, daughter of Edward D. Woodruff, a well known merchant of Philadelphia. During his directorship of the United States Geological Survey of the Territories they lived quietly at Washington, but upon his retirement they went to live in Philadelphia. No children blessed this union, but their devotion to each other was of the most sincere and earnest kind.

After his resignation from the survey Dr. Hayden's health failed rapidly and it soon became apparent that his end was rapidly approaching. A recent letter to me from Mrs. Hayden contains the following touching statement concerning his condition during the last year of his life: "During the year, with the exception of two months in the summer, when he was carefully taken in a carriage for a visit to Bryn Mawr, he was confined to his room. His library was utilized for his greater comfort, and here he sat so helpless as

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to require the constant attention of a man-servant, but thankful that he could always hold his books and that he was able until the last to use his eyes. Those who visited him there felt that it was a never-to-be-forgotten scene. The unusual sweetness and cheerfulness of his nature, united with a marvelous strength of self-control, were never more strongly marked. Speaking but little of himself, his pain, or of the disappointment at the loss of his active life, he was ever ready with a cheerful greeting to his old friends, taking the greatest interest in literary matters of the day and in the affairs of the world, from which he seemed shut away."

He died on December 22, 1887, and his body lies buried in Woodland Cemetery, in West Philadelphia. His monument is an unpretentious one of granite, surmounted by a conventionalized Greek cross, with a circle, below which is the following inscription :

FERDINAND V. HAYDEN, M. D.,
Died December 22nd, 1887.

Below this is a space reserved for Mrs. Hayden's name, and beneath that space is the motto :

Fide et Amore.

My long personal acquaintance with Dr. Hayden entitles me to say something of his personal qualities, but I prefer to quote and endorse the expressions of a few others of his friends. Dr. A. C. Peale says of him : "The apparent diffidence which impressed Dr. Hayden's fellow-students at Oberlin and led them to be doubtful as to his future course in life characterized his maturer years, and to those not well acquainted with him made it difficult to account for his success. However, enthusiasm, perseverance, and energy were qualities equally characteristic of him all his life, and what seemed to be diffidence was largely the result of his nervous temperament. The secret of his success is to be found in his enthusiastic frankness and his energetic determination to carry through whatever he undertook. He was absorbed in the work of the Geological Survey and bent all his energies to its success. Excitable in temperament and frequently impulsive in action, he was generous to a fault, and although ever ready to defend what he believed to be right he stood ready upon the presentation of evidence to modify his views.

"He was always careful to give due credit to all who had worked in the fields he afterwards explored. In one of his reports, speaking of those who had preceded him, he says: 'Any man who regards the permanency or endurance of his own reputation will not ignore any of those frontiersmen who made their early explorations under circumstances of great danger and hardship.'

"The same spirit actuated him in his treatment of his subordinates and coworkers. His honesty and integrity were undoubted and his work for the Government and for science was a labor of love."

Sir Archibald Geikie, Director of the Geological Survey of Great Britain, in a sketch which he published in "Nature" says: "There can be no doubt that among the names of those who have pioneered in the marvelous geology of western North America that of F. V. Hayden will always hold a high and honored place. This place will be his due, not only because of his own personal achievements in original exploration. His earlier work exhibits much of that instructive capacity for grasping geological structure which is the main requisite for a field geologist. He had a 'keen eye for a country,' but he likewise possessed the art of choosing the best men for his assistants, and the tact of attracting them to himself and his corps. In this way he accomplished much excellent work, keeping himself latterly in the background so far as actual personal geological investigations were concerned, and contenting himself with the laborious task of organization and supervision, while he encouraged and pushed forward his coadjutors."

Prof. J. P. Lesley, a member of this Academy, who had known him intimately for many years, paid the following tribute to his memory in a paper read before the American Philosophical Society: "He represented in science the curiosity, the intelligence, the energy, the practical business talent of the western people. In a few years they came to adopt him as their favorite son of science. He exactly met the wants of the Great West. There was a vehemence and a sort of wildness in his nature as a man which won him success, coöperation, and enthusiastic reputation among all classes, high and low, wherever he went. In the wigwam, in the cabin, and in the court-house he was equally at home, and entirely one with the people. He popularized geology on the grandest scale in the new States and Territories. He easily and naturally affiliated with every kind of explorer, acting with such friendliness and manly

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justice toward those whom he employed as coworkers that they pursued with hearty zeal the development of his plans.

"In dealing with the public men of the country he was so frank, forcible, and direct that it was impossible to suppress or resist him. He had the western people at his back so heartily and unanimously that he was for a long time master of the scientific situation at Washington. He was a warm personal friend of some of the highest officials of the Government, who never failed to support strenuously and successfully his surveys."

The recognition of Dr. Hayden's services to science was so general that he was elected to membership in most of the scientific societies and academies of this country, and to about seventy of those of foreign countries. The degree of LL.D. was conferred upon him by the University of Rochester and the University of Pennsylvania; the former in 1876, and the latter in 1886. He was elected to membership in this Academy in 1873.

When a man has devoted his life to some special branch of science it is comparatively easy for some one to write an exhaustive and accurate estimate of the value of his labors, upon the basis of his published works; but the writings of Dr. Hayden alone do not afford the means of properly estimating the value of his services to science. He wrote no exhaustive treatises, but he made it possible for many others to prepare and publish such, and the scientific reputation of a considerable number of men was largely accomplished by means of the opportunities which he created. Nearly fifty persons whose names have become known to science were from time to time more or less directly connected with the surveys under his direction, no less than thirteen of whom are, or were when living, members of this Academy.

I do not by any means intend to intimate that Dr. Hayden's published writings and his observations in the field are of an inferior kind. Indeed, no geologist can fully review his work without being impressed with the accuracy of many of his generalizations, the fewness and unimportance of his mistakes, and the number of instances in which he anticipated opinions concerning the import of facts which he observed, which have been advocated by others. These facts will be all the more impressive if considered in connection with the wild and difficultly accessible character of the country which he traversed, the necessarily short time which during each journey he could devote to investigation in the field, and the gen-

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eral condition of geological science at the time he began his work. There is, in my opinion, no room for doubt that the labors of Dr. Hayden, directly or indirectly, accomplished more for the general advancement of geological science in America than those of any one else. Indeed, I think it may be truthfully said that the present United States Geological Survey is a consequent of his labors, and that it would not now be in existence if Dr. Hayden's work had remained unaccomplished.

For the reasons just referred to a bibliographical list of the writings of Dr. Hayden is quite inadequate as an index to the work which he actually accomplished. I think, however, it is appropriate that I should give at least a partial list, one which shall include the titles of such of his own writings as are not contained in the different series of official publications of the surveys which were under his direction. I therefore present the following list:

A PARTIAL LIST OF THE PUBLISHED WRITINGS OF
DR. F. V. HAYDEN,

WITH GENERAL REFERENCES TO ALL THE OTHERS.

1. Explorations under the War Department. Notes explanatory of a map and section illustrating the geological structure of the country bordering on the Missouri river, from the mouth of Platte river to Fort Benton, in lat. $47^{\circ} 30'$, long. $110^{\circ} 30' W.$ (Communicated by permission of the Secretary of War.) Proc. Acad. Nat. Sci. Phila., vol. ix, pp. 109-116. Philadelphia, 1858. Also issued as separates.

2. Notes on the geology of the Mauvaises Terres of White river, Nebraska. Proc. Acad. Nat. Sci. Phila., vol. ix, pp. 151-158. Philadelphia, 1858.

3. Explorations under the War Department. Explanation of a second edition of a geological map of Nebraska and Kansas, based upon information obtained in an expedition to the Black hills, under the command of Lieut. G. K. Warren, Top. Engineer, U. S. A. Proc. Acad. Nat. Sci. Phila., vol. x, pp. 139-158. Philadelphia, 1859. Also issued as separates.

4. Tertiary basin of White and Niobrara rivers. Am. Jour. Sci., vol. xxvi, pp. 404-408. New Haven, 1858.

5. Geological sketch of the estuary and fresh-water deposits of the Bad Lands of the Judith, with some remarks upon the surrounding formations. Am. Phil. Soc. Trans. Phila., vol. xi, pp. 123-128. Philadelphia, 1859.

6. Notes on the geology of Nebraska and Utah Territory (in a letter to one of the editors). Am. Jour. Sci., vol. xxix, pp. 433, 434. New Haven, 1860.

7. Sketch of the geology of the country about the headwaters of the Missouri and Yellowstone rivers. Am. Jour. Sci., vol. xxxi, pp. 229-245. New Haven, 1861.

8. The primordial sandstone of the Rocky mountains in the north-western Territories of the United States. Am. Jour. Sci., vol. xxxiii, pp. 68-79. New Haven, 1862. Also issued as separates.

9. Physics and hydraulics of the Mississippi river. Am. Jour. Sci., vol. xxxiii, pp. 181-189. New Haven, 1862. This article is a review of Humphreys and Abbott's report on the physics and hydraulics of the Mississippi river. Also issued as separates.

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10. Some remarks in regard to the period of elevation of those of the Rocky mountains near the sources of the Missouri river and its tributaries. *Am. Jour. Sci.*, vol. xxxiii, pp. 305-313. New Haven, 1862. Also issued as separates.

11. Colorado river of the West. *Am. Jour. Sci.*, vol. xxxiii, pp. 387-403. New Haven, 1862. This article is a review of Lieut. J. C. Ives' report upon the Colorado river of the West. Also issued as separates.

12. A sketch of the Mandan Indians, with some observations illustrating the grammatical structure of their language. *Am. Jour. Sci.*, vol. xxxiv, pp. 57-66. New Haven, 1862. Also issued as separates.

13. Contributions to the ethnography and philology of the Indian tribes of the Missouri valley. *Am. Philos. Soc. Trans.*, vol. xii, pp. 231-461, 1 map and 2 plates of illustrations. Philadelphia, 1862. Also issued as separates.

14. On the geology and natural history of the upper Missouri. *Am. Philos. Soc. Trans.*, vol. xii, pp. 1-218, and map. Philadelphia, 1862. Also issued as separates.

15. [Remarks on a visit to the great Red Pipestone quarry.] *Am. Philos. Soc. Proc.*, vol. x, pp. 274, 275. Philadelphia, 1866.

16. [Description of a chalk deposit in the valley of the Missouri river.] *Am. Philos. Soc. Proc.*, vol. x, p. 277. Philadelphia, 1866.

17. [Remarks upon the geology of the Missouri valley.] *Am. Philos. Soc. Proc.*, vol. x, pp. 292-296. Philadelphia, 1866.

18. [Remarks upon the Lignite beds of the country of the upper tributaries of the Missouri.] *Am. Philos. Soc. Proc.*, pp. 300-307. Philadelphia, 1866.

19. Sketch of the geology of northeastern Dakota, with notice of a short visit to the celebrated Red Pipestone quarry. *Am. Jour. Sci.*, vol. xliii, pp. 15-22. New Haven, 1867. Also issued as separates.

20. [Scarcity of timber in certain parts of the upper Missouri river region.] *Am. Philos. Soc. Proc.*, vol. x, pp. 322-326. Philadelphia, 1869.

21. [Geographical distribution of plants in the region west of the Mississippi.] *Am. Philos. Soc. Proc.*, vol. x, pp. 315-320. Philadelphia, 1869.

22. Remarks on the Cretaceous rocks of the West, known as No. 1, or the Dakota group. *Am. Jour. Sci.*, vol. xliii, pp. 171-179. New Haven, 1867. Also issued as separates.

23. Notes on the geology of Kansas. *Am. Jour. Sci.*, vol. xlv, pp. 32-40. New Haven, 1867. Also issued as separates.

24. [Observations concerning the Indians of the lower Missouri river region.] *Am. Philos. Soc. Proc.*, vol. x, pp. 352, 353. Philadelphia, 1867.

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25. [Results of geological exploration in Nebraska.] Report of the Commissioner of the General Land Office for the year 1867, pp. 128-131. Gov't Printing Office, Washington, 1867.
26. Rocky Mountain coal beds. *Am. Jour. Sci.*, vol. xlv, pp. 101, 102. New Haven, 1868.
27. Notes on the Lignite deposits of the West. *Am. Jour. Sci.*, vol. xlv, pp. 198-208. New Haven, 1868.
28. [Remarks on the Pawnee, Winnebago, and Omaha languages.] *Am. Philos. Soc. Proc.*, vol. x, pp. 389-421. Philadelphia, 1869.
29. Remarks on the geological formations along the eastern margin of the Rocky mountains. *Am. Jour. Sci.*, vol. xlv, pp. 322-326. New Haven, 1868.
30. Remarks on the possibility of a workable bed of coal in Nebraska. *Am. Jour. Sci.*, vol. xlv, pp. 326-330. New Haven, 1868.
31. Notes on the geology of Wyoming and Colorado Territories, No. 1. *Am. Philos. Soc. Proc.*, vol. x, pp. 463-478. Philadelphia, 1869.
32. Geological explorations in Wyoming Territory. Report of the Commissioner of the General Land Office for the year 1868, pp. 229-235. Gov't Printing Office, Washington, 1868.
33. Report on the geography of the Missouri valley. Report of the Commissioner of the General Land Office for the year 1868. Gov't Printing Office, Washington, 1868.
34. Notes on the geology of Wyoming and Colorado Territories, No. 2. *Am. Philos. Soc. Proc.*, vol. xi, pp. 25-56. Philadelphia, 1869.
35. A new species of Hare from the summit of Wind River mountains. *American Naturalist*, vol. iii, pp. 113-116. Salem, 1869.
36. On the geology of the Tertiary formations of Dakota and Nebraska. Dr. Joseph Leidy's Extinct Mammalian Fauna of Dakota and Nebraska, pp. 9-21, and map. Lippincott, Philadelphia, 1869.
37. Geological report of the exploration of the Yellowstone and Missouri rivers under the direction of Capt. W. F. Reynolds, Corps of Engineers, 1859-'60. 174 pp. and map. Gov't Printing Office, Washington, 1869.
38. Sections of strata belonging to the "Bear River Group," near Bear River City, Wyoming Territory. *Am. Philos. Soc. Proc.*, vol. xi, pp. 420-425. Philadelphia, 1870.
39. Preliminary field report of the United States Geological Survey of Colorado and New Mexico. *Am. Jour. Sci.*, vol. xlix, pp. 258-263. New Haven, 1870.
40. Sun pictures of Rocky mountain scenery, with a description of the geographical and geological features and some account of the resources of

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the Great West, containing 30 photographic views along the line of the Pacific railroad from Omaha to Sacramento. 4to, 150 pp. and 30 photographic plates. Julius Bien, New York, 1870.

41. Final report of the United States Geological Survey of Nebraska and portions of the adjacent Territories, made under the direction of the Commissioner of the General Land Office. 8vo, pp. 264, 1 map and 11 plates. Washington, 1872. (The volume is in large part paleontological, that work having been done by F. B. Meek.)

42. The hot springs and geysers of the Yellowstone and Firehole rivers. *Am. Jour. Sci.*, vol. iii, pp. 105-115, and maps. New Haven, 1872.

43. Die neu entdeckten Geyser-Gebiete am Yellowstone und Missouri River. Nach dem officiellen Bericht von F. V. Hayden; mit Karten und Ansichten. Dr. A. Petermann's Mittheilungen, 18 Band, Seiten 241-253, 1 Tafel. Gotha, 1872.

44. The wonders of the West. More about the Yellowstone. *Scribner's Monthly Magazine*, vol. iii, pp. 383-396. New York, 1872.

45. The Yellowstone National Park. *Am. Jour. Sci.*, vol. iii, pp. 294-297, and map. New Haven, 1872.

46. Rocky mountains. *American Cyclopædia*, vol. xiv, pp. 374-382. D. Appleton & Co., New York, 1874.

47. Our Great West, and the scenery of our natural parks. *Bull. Am. Geograph. Soc.*, No. viii, pp. 12-28. New York, 1874.

48. Summary of the field-work of the Hayden Geological Survey during the season of 1875. *The Republic*, a monthly magazine, for March, 1876, pp. 149-160. Washington, 1876.

49. Wonders of the Rocky mountains: The Yellowstone Park, how to reach it. *Pacific Tourist*, Williams' Illustrated Guide to the Pacific Railroad, California, etc. New York, 1876.

50. The Yellowstone Park and the mountain regions of portions of Idaho, Nevada, Colorado, and Utah. Folio, 48 pages, 2 maps, and 15 chromolithographic plates. Prang & Co., Boston, 1876.

51. [Explorations made under the direction of Prof. F. V. Hayden in 1876.] *The Republic*, a monthly magazine, for January, 1877, pp. 17-24. Washington, 1877.

52. [Progress of the United States Geological Survey under the direction of Prof. F. V. Hayden.] *The Republic*, a monthly magazine, for December, 1877, pp. 217-226. Washington, 1877.

53. The field-work of the United States Geological Survey of the Territories, under the direction of Prof. F. V. Hayden, for the season of 1877. *American Naturalist*, vol. xii, pp. 97-114. Salem, 1878.

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54. Sketch of the origin and progress of the United States Geological and Geographical Survey of the Territories. 15 pp., 8vo. Darby & Duval, printers, Washington, 1877.

55. Yellowstone National Park. Johnson's Universal Cyclopædia, vol. ix, pp. 1526-1530. Alvin J. Johnson & Son, New York, 1878.

56. The field-work of the United States Geological and Geographical Survey of the Territories, under the direction of Prof. F. V. Hayden, for the season of 1877. *American Naturalist*, vol. xii, pp. 96-114. Philadelphia, 1878.

57. [Letter to Hon. Carl Schurz, Secretary of the Interior.] Letter from the acting president of the United States National Academy of Sciences, transmitting a report on the surveys of the Territories. House of Representatives Mis. Doc. No. 5, pp. 11-13.

58. Discovery of recent glaciers in Wyoming. *American Naturalist*, vol. xii, pp. 830, 831. Philadelphia, 1878.

59. Wasatch group. *American Naturalist*, vol. xii, p. 831. Philadelphia, 1878.

60. Stanford's compendium of geography and travel, based on Hellwald's "Die Erde und ihre Völker." North America. Edited and enlarged by Prof. F. V. Hayden, late Chief of the United States Geological Survey, and Prof. A. R. C. Selwyn, F. R. S., Director of the Geological Survey of Canada. 652 pp., 17 plates, and 16 maps. London, 1883.

All the other known published writings of Dr. Hayden are contained in the following series of official reports: Annual Reports of the United States Geological Survey of the Territories, 1st to 12th, inclusive, 8vo; the quarto series of the same, vols. i to xiii, inclusive, except vol. iv, not published; bulletins of the same and miscellaneous publications of the same; also the annual reports of the present United States Geological Survey.

There are also omitted from the foregoing list all the publications which have appeared under the joint names of F. B. Meek and F. V. Hayden, a full list of which has been published in Bulletin No. 39 of the United States National Museum.

MEMOIR
OF
LEWIS MORRIS RUTHERFURD.
1816-1892.

BY
B. A. GOULD.

READ BEFORE THE NATIONAL ACADEMY, APRIL, 1895.

MEMOIR OF LEWIS MORRIS RUTHERFURD.

It is somewhat more than two years since I received notice of my designation for the duty of preparing the biographical notice of our late colleague, Lewis M. Rutherford. It is an honoring duty, which could not fail to be most welcome, provided only that it could be fitly performed. Unhappily the possibility of its performance has depended upon circumstances beyond my control. Having been a constant and deeply interested observer of the successive steps by which Mr. Rutherford had given impulse to the progress of more than one branch of astronomy, leaving upon it the impress of his thought and ingenuity, and having been at the same time privileged to call him friend, it would have been a source of pain to me had the duty been assigned to another; and under whatever contingency it would have been regarded as paramount to any within my option. That it has been so long deferred has been due simply to my inability to undertake it, and even now it has been necessary to elect between the alternatives of relinquishing the task or of fulfilling it in a manner unsatisfactory to myself.

The most marked mental characteristics of this gifted and beloved man were clearness of comprehension, independence of judgment, and unselfishness of purpose. With these were combined rare mechanical ingenuity and inventive power, and the character was rounded out by learning, by the culture of a refined gentleman, and by an exceptional absence of all self-assertion. For knowledge of his achievements one might search the newspapers and popular magazines in vain, for he did not covet the applause of the multitude or other approval than that awarded by fully competent judges.

Mr. RUTHERFURD was born at Morrisania, New York, now a portion of New York city, on the 25th of November, 1816, being the son of Robert Walter Rutherford and Sabina Elliott Morris. His paternal grandfather, John Rutherford, represented New Jersey in the United States Senate from 1791 to 1798, being a son of Walter Rutherford, who had previous to the Revolution resigned a commission in the British army, become a citizen of New York, and

married a daughter of James Alexander and sister of Major General William Alexander, known in American history as the Earl of Sterling, who commanded the left wing of our army at Monmouth. On the maternal side his ancestor of the sixth generation was Lewis Morris, chief justice of New York and New Jersey and the first governor of New Jersey. The great-grandfather, for whom he was named, was the signer of the Declaration of Independence.

Our late colleague studied at Williams College, whence he graduated in 1834, at the age of 18 years. While an undergraduate his inborn tastes for physical research and mechanical contrivance manifested themselves in a conspicuous degree, and he acted as assistant to the professor of physics and astronomy, preparing the experiments and constructing apparatus. After pursuing the study of law for two years in the office of William H. Seward, at Auburn, and afterwards in that of George Wood, in New York city, he began the practice of his profession as partner of Mr. Peter A. Jay, and subsequently continued it as partner of the late Hamilton Fish.

But, from the first, astronomy had for him a higher charm than legal studies, and after fortunate circumstances relieved him from the necessity of seeking pecuniary gain, he devoted himself more and more to scientific study.

In July, 1841, he had married the accomplished Margaret Stuyvesant Chanler, niece and adopted daughter of Mr. Peter Stuyvesant, husband of Mr. Rutherford's aunt. This happy union, which continued for almost half a century, was an indirect source of many benefits to science, for it afforded him inestimable encouragement and sympathy within his household, while the same were cordially extended to the associates of his researches. His uncle and aunt were more than glad to aid him in his scientific undertakings, and the ancestral house and garden in the midst of what had grown to be a densely populated portion of the great city gradually became a nucleus of scientific research. An observatory, with an 11 $\frac{1}{4}$ -inch telescope constructed by Fitz under Mr. Rutherford's immediate guidance and supervision, and a good transit-instrument afforded opportunity for sundry researches of a higher character than those of a mere amateur, and later a workshop was gradually equipped with apparatus which he employed for the construction of instruments the precision of which was hardly, if at all, surpassed by any in the land.

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While keeping up his law practice these early scientific labors were pushed forward with astonishing energy, and his health was maintained by the practice of sundry athletic sports, for he was an enthusiastic yachtsman and a champion racquet-player, and was actually on board the "America" during her celebrated race in English waters.

But soon after his withdrawal from practice of the law it became manifest that his wife's health demanded a complete change of climate and surroundings, and he entered upon a period of extended travel on the European continent. For several years he resided in cities of France, Germany, and Italy, gathering copious stores of scientific experience and becoming more familiar with the languages of these countries; so that when he returned home with renewed strength and energy he was ready to resume the scientific life for which he yearned.

During his residence in Florence he maintained intimate relations with Amici, who was then carrying on his experiments upon the achromatism of objectives for microscopes, his mechanician having been originally an umbrella-maker, whom Amici had gradually transformed into an admirable optical artist. Possibly this episode may account for a series of experiments which was undertaken by Mr. Rutherford more than a quarter of a century later, when he endeavored to apply, to the improvement of microscopic objectives, methods of local correction such as he had so brilliantly employed for telescopes.

His observatory in New York city served as a primary station for the determination of longitudes, being occupied, at his invitation, for this purpose by the Coast Survey on many occasions. We find "Stuyvesant garden" among the first points occupied by Sears C. Walker in his measurements of longitude.

It was in the last half of 1856 that his observatory was built and arranged, and in 1858 he began his experiments in astronomical photography. During the summer of that year he obtained negatives of the full moon which would well bear an enlargement of fifty diameters, and stereographs which gave excellent effects. This was at about the date at which De La Rue also obtained stereoscopic* results. I have been unable to decide which of the two preceded in this matter. At any rate, neither of them could have

*M. N., xvii, 257.

been much in advance of the other, and certainly each of them did the work independently. The earliest of Mr. Rutherford's lunar photographs preserved at Columbia College bears date June, 1858.

In the course of the next following year, 1859, he made extended studies for devising combinations of lenses for insertion in the tube of the telescope in order to make the light received from the object glass available for photographic purposes. Yet this proved successful only for the central portion of the field unless lenses of inconvenient size were introduced. Still, he did succeed to the extent of preparing apparatus for photographic observation of the total solar eclipse of 1860 in Labrador. Using for this an object-glass of about $4\frac{1}{2}$ inches (108 millimeters) and separating the crown and flint lenses by such an interval as should afford the best focus for the visual and chemical rays jointly, he obtained solar images decidedly superior to those afforded by his large telescope of $11\frac{1}{2}$ inches aperture. This was undoubtedly the first employment of a telescope constructed especially for photographic purposes, and on this occasion the difference in character of the limbs of the sun and the moon was first made distinctly and unmistakably manifest; that of the sun being hazy and indefinite, while that of the moon, in the same field, was sharp and with well-defined details of the serrated edge.

In 1861 he constructed a Cassegrainian reflecting telescope, with silvered glass mirror, having 13 inches aperture and 8 feet focus. It was figured by Mr. Henry Fitz, under his supervision, and weighed less than 15 pounds; but the silvering was so easily attacked by the abundant gases of the city and the moisture of the coast that he found it absolutely necessary to resilver the speculum at least as often as every ten days, and the tremors inseparable from the location in the heart of the great city opposed a fatal objection to good work, being about thirty-six times as large as in the achromatic telescope, where they gave no trouble. His employment of a reflector was consequently abandoned after about three months of trial.

It was at this time, however, that Dr. Draper was trying the same experiments with his silvered specula which gave such beautiful results in the dry, pure air of his observatory in the country.

It may be here remarked that the American process of figuring, or locally correcting, lenses of telescopes had been invented and used by Mr. Fitz, of New York, twenty years previously, and from asso-

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ciation with him Mr. Rutherford obtained a knowledge of all its details; these he in turn taught to Henry Fitz, son of the inventor, who for a couple of years was a nightly visitor of his workshop and observatory.

Notwithstanding the earnest zeal with which Rutherford prosecuted his astronomical and physical observations—keeping pace with the advances of celestial discovery—his modest disposition prevented him from publishing his results and thus taking the position properly belonging to him among investigators. It was not until the year 1862 that he published his first astronomical paper,* a letter dated 1862 July 28, confirming Clark's discovery, with his new 18½-inch object-glass, of the companion of Sirius and giving measures of its position on seven dates, from March 11 to April 10 of that year. In the evening of the day on which he received news of the discovery he found the object with his 11-inch telescope, and long afterwards he stated that he had never looked for it in vain. If I am not mistaken, it was not until at least two years later that it was seen with sufficient distinctness to be measured by any other astronomer, using an object-glass of less aperture than 15 inches. The difficult measurements of distance were accomplished by him with exceptional precision and contributed largely to our early knowledge on the subject. During the next season he secured seventy-nine measures of position-angle on six different nights, and thirty-eight of distance, and he reported that he had seen it in February, 1863,† with a telescope of 9 inches aperture, also made for him by Fitz. These observations, with those made with the 15-inch telescope at Cambridge and on two nights at Pulkowa, afforded the chief basis for our knowledge of the new-found body during the first two years after its discovery. Whether it shines by its own light or by reflection from the flood of intense light emitted by its brilliant neighbor is yet an open question, although the researches of Auwers‡ have shown that its mass is not much less than one-half [0.47] that of the primary, round which it revolves in an eccentric orbit and a period of about 49½ years (49.4).

But during the year 1862, in the interval between these series of measurements, Mr. Rutherford entered upon another investigation,

* A. J. Sc., 34: 294.

† A. J. Sc., 35: 407.

‡ A. N., 129: 185.

with results which proved to be of high importance, and may, indeed, be regarded as inaugurating the now widely cultivated study of astrophysics.

In 1814-'15 Fraunhofer had published the memoir containing his discovery of the remarkable lines in the solar spectrum, which offered a problem unsolved by physicists until nearly half a century later. A few of these seem, indeed, to have been seen by Wollaston in 1802, but were supposed by him to form boundaries between primary colors of the spectrum, and neither he nor any other physicist gave them special attention; but Fraunhofer, while studying the laws of refraction and dispersion by different kinds of glass with reference to the improvement of achromatic telescopes, instituted experiments for determining whether their action upon sunlight was the same as upon the lamplight that he had been using.

In the memoir* cited he describes the apparatus by which a small beam of solar light was thrown upon a flint-glass prism placed upon a theodolite. In the place of an especially brilliant beam between the red and the yellow, he was surprised to find a vast number of dark lines athwart the spectrum. The most marked of these he specifies, denoting them by letters—the same, indeed, by which they are still designated. Examining similarly the light of the planets Venus and Mars, but without limiting the beam by any aperture, he found in their spectra lines similar to those which had appeared in solar light, although they were less markedly defined.

It was but natural to extend the inquiry to the light of sundry bright fixed stars. For a minute investigation of these, he increased the width of the spectrum by placing in front of the objective a plate of glass having one surface plane and the other slightly cylindrical. Making the axis of this parallel to the base of the prism, the length of the spectrum remained unchanged, while its breadth was increased sufficiently to enable him to study the lines with care. In the spectrum given by Sirius he found three broad bands totally dissimilar to those found in the solar spectrum and measured their positions. Then, passing to other stars—Castor, Pollux, Capella, Procyon, and α Orionis—he was able to recognize dark lines in the spectra of these also, although apparently different for each star.

Thus the question of stellar spectra remained for forty-seven years.

* Denkschr. d. Münch. Akad., V, 193; Schum. Abh., II, 13.

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Rutherford's second scientific paper was dated December 4, 1862, and published in the *American Journal of Science** for January, 1863. It is entitled "Astronomical Observations with the Spectroscope" and begins as follows :

"In the course of a conversation last December [*i. e.*, 1861] with Doctor Gibbs upon the remarkable revelations of the spectroscope, he suggested the continuation of Fraunhofer's observations upon the spectra of the heavenly bodies. At that time I had not seen Fraunhofer's paper on this subject. I immediately began a series of experiments with the view of determining the best form of instrument for the purpose," etc.

He proceeds to describe his apparatus, which was attached to the eye-tube of his large Fitz telescope. At first he found it necessary to throw the star out of focus in order to obtain a sensible breadth of spectrum, and the observations were thus subjected to the disadvantage of losing a large portion of the light upon the jaws of the slit ; but, after obtaining a copy of Fraunhofer's memoir, he naturally availed himself of the device of a cylindrical lens, interposing this between the prism and the objective of the telescope and moving the slit to the focus, where no light was lost.

That even then he had already planned the photographic method of observation, by which he subsequently attained such distinction, is manifest from an incidental remark† made by him in this connection :

"I would here incidentally remark that the spectroscope, so mounted, furnishes the best means of investigating the achromatic condition of the telescope ; for it is evident that if the different-colored rays have foci at different distances from the objective, it will require a change of focus for each in order that it may comply with the condition of passing the slit (in case of a star) as a point. In my telescope I find that when the luminous rays from near the outer margin of the red to the indigo are brought to a focus at one point, the ultra red require a small but measurable adjustment, and the violet and indigo quite a large change of focus. I intend to make use of this method to find more accurately the photographic focus of the instrument, which at present is determined only by experiment."

In this paper he gives the results of his observations and measurements not only for the Sun, Moon, Jupiter, and Mars, but also for

* Vol. 35, p. 71.

† Amer. Jour. Sci., 35 : 72.

seventeen fixed stars, as well as accounts of the examination of six others. He concludes as follows :

"The star spectra present such varieties that it is difficult to point out any mode of classification. For the present I divide them into three groups: First, those having many lines and bands and most nearly resembling the sun, viz., Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the sun, and are white stars. The third group, comprising α Virginis, Rigel, etc., are also white stars, but show no lines; perhaps they contain no mineral substance or are incandescent without flame.

"It is not my intention to hazard any conjectures based upon the foregoing observations—this is more properly the province of the chemist—and a great accumulation of accurate data should be obtained before making the daring attempt to proclaim any of the constituent elements the stars.

"One thought I cannot forbear suggesting: We have long known that 'one star differeth from another star in glory;' we have now the strongest evidence that they also differ in constituent materials—some of them perhaps having no elements to be found in some other. What, then, becomes of that homogeneity of original diffuse matter which is almost a logical necessity of the nebular hypothesis?

"Taking advantage of past experience, I propose to remodel and improve my spectroscope and continue to observe the stars, noting particularly the relations which may exist between the spectral revelations and the color, magnitude, variability, and duplicity of the objects."

I cannot forbear calling attention to the classification, essentially the same, subsequently published by Secchi without reference to this or to any of the other labors of Rutherford, and which is still generally cited* under Secchi's name.

Yet while these researches of Rutherford were not only independent, but were actually brought to the notice of astronomers in general at an earlier date than those of any other investigator since Fraunhofer, it must be conceded that they were antedated by analogous ones of Professor Donati at Florence, published in the *Annali del Museo Fiorentino* in the same year, 1862, the earliest being dated, indeed, in August, 1860. The Italian astronomer, using a large burning glass, kept in the museum at Florence since 1690, had essentially employed the method of Fraunhofer, with a translation of whose memoir he was familiar. The great lens being paral-

* See Scheiner, p. 258, and Translation, pp. 235, 236.

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lactically mounted on a movable stand, a screen with a narrow slit was placed just inside of its focus and a cylindrical lens then interpolated between the image of the star and the prism. The spectrum was then viewed with a telescope. In this way Donati examined the light of fifteen stars and roughly described the prominent lines of their spectra. He stated that all these showed a line differing but little from the solar F, but modified somewhat in its position by a difference in the refractive quality of the light from the particular star.

It may fairly be asserted that Rutherford's independent results far excelled the others in comprehensiveness, minuteness of detail, and accuracy of measurement.

About four months later, at the close of March, 1863,* when publishing his second series of observations of the companion of Sirius, he states that he had mounted his astronomical spectroscope more conveniently and had added a prism, by which the spectrum from a lamp was kept in the field of view during the observation of a star. He thus established the existence of the lines D, E, *b*, and G in the light of Arcturus, and found that each line in the spectrum of that star had its counterpart in the solar spectrum.

In June of the same year, under the title of "Observations on Stellar Spectra," Mr. Rutherford called attention† to the results which had then been obtained by European investigators of the subject. He described the several forms of apparatus which they had employed, calling attention to their respective advantages and disadvantages and suggesting how the latter might be remedied. It will surprise no one who knew Rutherford, when I mention that in the whole paper there is not the slightest mention of any claim to priority, or allusion to his own previous results, except in one instance, where he modifies a previous announcement that he had found nine lines in the solar D by stating that five of these had since proved to be telluric.

From this time on, the study of the spectra of celestial bodies has gone steadily forward. It has become a favorite pursuit with a large class of students, to such an extent, indeed, as to absorb much of the energies which might otherwise have been devoted to the more laborious and exacting investigations of astronomy proper ;

* A. J. Sci., 35: 407.

† Am. Jour. Sci., 36: 154.

but we cannot wonder at the fascination exerted by the new class of previously unsuspected revelations which we owe to this wonderful method of inquiry and from which astronomy is justified in expecting highly important contributions.

During the winter of 1862-'3 he had likewise constructed a large spectroscope, with telescopes of 4 centimeters [1.6 inches] aperture and 50 centimeters [20 inches] focal length, having six hollow glass prisms filled with bisulphide of carbon. This presented great technical difficulties, both in the attainment of plane parallel surfaces and in the quality of the bisulphide.

Yet it was not until the close of 1864 that he described* this instrument in its perfected form. In this, the observing telescope had two motions, one about the central axis of the instrument and the other about a second vertical axis, the whole being so arranged that the entire spectrum could be commanded by a single motion. Various devices were tried to secure equal density in the carbon bisulphide independent of thermal variations, and the very serious difficulties encountered in this respect were successfully overcome by methods still employed when prisms of this sort are required.

Finally, by an ingenious contrivance, he succeeded in avoiding all necessity for the laborious adjustment of the angle of least deviation for the several prisms by the mechanical arrangement, now well known, in which the prisms are hinged together at their corners and so connected with a central axis that all of them are, under all circumstances, kept in such a position that their backs are tangent to the same circle. By a motion of a milled head at the central axis all the prisms are, as has been stated, made to approach or recede from this axis, their backs thus becoming tangent to a larger or a smaller circle, and the adjustment can thus be varied as required for examining different parts of the spectrum.

Perhaps few of the observers, who now employ apparatus made on this principle, are aware that it was devised more than thirty years ago.

Rutherford lost no time in making this apparatus serviceable for photographic purposes, and in thus obtaining at an early date a magnificent representation of the solar spectrum, especially of the portion between the lines H and F, although it extended further. This exhibited three times as many lines as were shown on the map

* A. J. Sci., 39: 129.

of Bunsen and Kirchhoff, and was described in a communication which he presented to this Academy in January, 1864, when he exhibited impressions measuring 78.7 centimeters between the lines mentioned and 15 centimeters in width.

This brilliant achievement, which the older members of the Academy will remember, he neglected to put on printed record in due form. The National Academy was then in its infancy, and issued no series either of proceedings or of memoirs, and Mr. Rutherford himself was too much absorbed in the further prosecution of his researches to devote much of his time to narrating his own successes. The photograph spoke for itself and remained in existence, although it seems not to have been fully described in print until nearly two years later, when Müller published an account of it in Poggendorff's *Annalen*,* November, 1865. This disregard of public applause was characteristic of the man.

In this connection I may mention another very marked illustration of the same quality, afforded by his neglect to write out and publish his remarkable communication to the Academy at Northampton in August, 1866, in which he gave an oral account, in full detail, of the processes by which he had obtained the then wonderful photographs of stellar clusters, of the micrometer which he had constructed for measuring them, and of the measurements already obtained. The memoir by the present writer, containing the computation of the results, and read before the Academy on the same day, was naturally withheld from publication until Mr. Rutherford should have published his own paper describing the means and methods by which the data had been obtained. Rutherford's communication, was, however, never put in shape for the printer, and the other one remained unpublished for twenty-two years, after which interval it was printed from the original manuscript. Meanwhile the art of photography had been wonderfully developed and advanced and its application to stellar astronomy become a more common thing; although, to be sure, no other accurate measurements of relative positions thus determined had even then been made public. Indeed, but for a letter sent September 3, 1866, to the editor of the *Astronomische Nachrichten*, containing the resultant positions, relative to Alcyone, of the ten other brightest stars of the Pleiades and the incidental mention therein of Rutherford's

*126, p. 435.

labors, no printed evidence would have existed of what he had then accomplished. Perhaps this may have been carrying his modesty somewhat too far.

The methods and their results attracted little notice from astronomers, and that little was chiefly in the way of general objections to numerical deductions from measurements of photographs. It was not until long afterward that it became generally conceded that, when employed with due precautions and under proper conditions, photography affords a mode of observation inestimably superior in convenience and certainly equal in accuracy to any measurements which could be made directly with the telescope. More detailed mention of the memoirs referred to and of the delay in their publication may be found in the Proceedings of the American Association for the Advancement of Science, at the Buffalo meeting, in 1886. It is not strange that these results should have been easily forgotten and ignored in foreign countries; yet it is sad, if not mortifying, that they should be overlooked by American writers on the subject at the present day.

To return, however, to the narrative of his photography of the spectrum, it was soon after the attainment of his results with the bisulphide prisms in 1863 that Rutherford became convinced of the possibility of obtaining yet better ones by the employment of a diffraction grating.

The best fine-ruled plates existing at that time were those made by Nobert at Greifswald, and largely employed for studying the phenomena of interference and determinations of wave-lengths. Rutherford determined to prepare some glass plates of this sort and adapt them to a spectrometer. Nobert had succeeded in ruling a few small groups of lines on glass as tests for microscopes* with about 296 lines to the millimeter—*i. e.*, at intervals of less than 3.4 microns—while for less severe test-objects intervals of eighteen microns sufficed; but his method of ruling these and the diffraction gratings was jealously guarded as a "trade secret;" so that Rutherford was compelled to devise and test his own methods, having, however, satisfied himself that Nobert's plates were not made by the aid of a screw, since their errors were not periodic, but quite irregular.

After numerous preliminary experiments he constructed a small

* Pogg. Ann., 1852, lxxxv, 80.

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motor, with a little turbine impelled by the water from the city pipes, and fitted this up in his study in such way that its action should go on continuously, day and night, under sufficiently constant pressure. A diamond point traced parallel lines upon a glass plate pushed regularly forward by a system of levers acting on an acute glass wedge, this in its turn pushing the plate sideways. The gratings thus obtained were admirable, except that from time to time, after two or three hundred lines had been traced, some change would occur, producing a new series of lines with a slightly different interval. This he attributed at first to imperfect action of the pushing levers; but his friend, Professor Rood, who was deeply interested in the undertaking, prepared an optical-interference apparatus, by means of which it was discovered that the trouble was due to variations in the friction of the apparatus employed. This obstacle it was found impossible to remove; and in 1867 he constructed an elaborate ruling machine in which the plate was moved by a screw. Upon the construction of this he expended a very large amount of labor, and with it he made several beautiful gratings, although even these were not entirely to his satisfaction. The diamond points were made by a machine which he constructed for the purpose, and were conical; but they proved not to be durable, and he therefore substituted wedge-shaped edges, a form which he always used afterwards. By the study of the plates themselves he deduced the nature and amount of the periodic error of the screw and devised means for its correction. Thus, after the lapse of another year, all his difficulties with the machine, including those due to variations of temperature, had been overcome by patient and long-continued study of its products; and many gratings upon glass were ruled which gave no room for criticism, and left those of No-bert far behind. The toothed wheels for this machine he made himself, upon a dividing circle more than two feet in diameter, which he purchased but personally refitted. In Professor Rood's laboratory is an excellent grating on glass, with 255 lines to the millimeter, bearing the date 1870. In 1875, or earlier, he silvered the gratings with a view to their more convenient spectroscopic use, and Professor Rood has exquisite specimens of these, measuring about 16.4 millimeters by 24.5 millimeters and with 11161 lines at intervals of 680.4 to the millimeter.

Later still, similar gratings were made upon speculum metal, in

order to avoid the great wear upon the diamond, and Mr. Chapman, his assistant, produced a large number of these.

The ruling machine was enlarged in 1877, so as to trace lines 43 millimeters long, and with this Mr. Chapman ruled several gratings of a quality which seems yet unsurpassed, although the best ones of Professor Rowland and Mr. Brashear contain a much larger number of lines. But they are now superseded for certain purposes by Professor Rowland's admirable device of five or six years later, according to which the gratings are ruled upon a concave surface.

With these gratings adjusted for spectroscopic use Rutherford obtained, from 1867 on, visual and photographic results for the study of both solar and stellar light, which commanded universal admiration and were not equaled until those of Draper * many years later.

But, important as were Rutherford's contributions to spectral analysis, these are entirely overshadowed by his epoch-making services in connection with celestial photography.

In his paper † of December, 1862, he had called attention to the peculiar adaptability of the spectroscope for investigating the achromatism of an object-glass and spoken of his intention to employ this device for determining the photographic focus of his telescope. The form of the spectrum from a star or any point of white light shows at once what colors are, and what are not, brought to a sharp focus; since in the one case it is simply a line, but in the other its width is expanded to that of the cone of rays where intercepted by the prism. Experimenting with the aid of this fact, he found that a good practical focus for the chief rays needed in celestial photography was attainable by separating the crown and flint lenses by such an interval that the focal length of the combination should be about one-twentieth shorter than that required for visual achromatism, and that, while such a separation of the lenses would be entirely unsuitable for vision, it would afford photographic impressions of a sharpness and nice definition which he had not succeeded in attaining otherwise. Still, since this naturally implied a mere compromise for convenience, by using lenses of such figure that part of the rays employed for either visual or photographic purposes must be sacrificed, he finally resolved to construct an object-glass

* A. J. Sci., xviii, 419.

† A. J. Sci., xxxv, 72.

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adapted to photographic use only. This presented a very embarrassing problem, since the needful corrections for figure could not be determined by the judgment of the eye alone unless the more brilliant but less refrangible rays should be excluded. To attain this end his first essays were made by interposing colored solutions to absorb most of the undesirable rays. These, however, were found to obstruct too many of those rays which were required, and it thus became necessary to employ long and tedious photographic processes for determining the local corrections of the object glass for figure; yet these were considerably facilitated by using the spectroscopic method of examination for focus.

At last, toward the close of the year 1864, his patient labors were rewarded by the completion of a well-figured object-glass, constructed solely for photographic use. It had an aperture of $11\frac{1}{4}$ English inches, just equal to that of his existing achromatic, and was so mounted that either of the two could readily be substituted for the other in the same tube.

With this telescope, made by himself, with the younger Fitz as assistant, stars nearly, if not quite, as faint as the ninth magnitude were easily photographed by an exposure of three minutes, and beautiful impressions of the Pleiades and the Præsepe were obtained, showing in the former thirty-two stars and in the latter thirty-one; also another of the cluster in Perseus. On the evening of March 6, 1865, he made with it the exquisite photograph of the moon which is so well known and which long remained far superior in distinctness of definition to any others existing. Not until Draper had constructed and employed his 28-inch reflector was it equaled. The sharpness of the images is shown by his account published in the *American Journal of Science** of May, 1865:

"Since the completion of the photographic objective but one night has occurred (March 6) with a fine atmosphere, and on that occasion the instrument was occupied with the moon, so that as yet I have not tested its powers upon the close double stars, $2''$ being the nearest pair it has been tried upon. This distance is quite manageable provided the stars are of nearly equal magnitude. The power to obtain images of the ninth-magnitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens and in some measure to realize the hopes which have so long been deferred and disappointed.

* xxxix, 308.

"It would not be difficult to arrange a camera-box capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved and more sensitive methods devised."

In this connection it will, of course, be remembered that the dry plate and bromogelatin processes were not introduced until fourteen or fifteen years later; so that, not to speak of the far greater sensitiveness of these later plates, the removal of the limitation to the period of exposure, imposed by the condition that their moisture should not evaporate, makes comparisons between present results and those earlier ones almost absurd. At the time of Rutherford's experiments the duration of an exposure was restricted to three or four minutes in such states of the atmosphere as usually prevailed when the sky permitted good photographic work, especially in our relatively dry climate. The present custom of celestial photographers, who continue the exposures for a single impression through a series of nights, and these not necessarily consecutive, would have seemed as incredible as the telephone or the kinetoscope.

Beside a very large number of photographs of the sun and moon, many of them during eclipse, Rutherford took during the years 1865 and 1866 twenty-five impressions of the Pleiades, five of the Præsepe, six of the great cluster in Orion, and several of double and multiple stars, all with trails for determining the zero of position-angle.

But the mere attainment of these admirable impressions really accomplished no more for astronomical science than the preservation in permanent form of the configurations which a small telescope would exhibit to the eye. No one was more fully aware of this than the gifted and ingenious physicist whose devices and mechanical skill had secured the photographic records. To give them their scientific value they must be measured upon the plates with a careful accuracy analogous to that employed in delicate micrometric measurements in the telescope. This was his next task.

The special obstacles to be encountered in effecting these measurements were, of course, to be discovered by experiment, and for this purpose Rutherford planned and constructed, chiefly in his own workshop, a micrometric apparatus so arranged as to permit the measurements to be made in various ways. It was designed on

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the principle of the slide-rest of a lathe, a vertical microscope being carried over the photographic plate by either of two micrometer screws at right angles to each other, while the plate rested near the center of a graduated horizontal circle, which could be rotated within a concentric and fixed one, provided with verniers. Of course, no pains were spared in the micrometer-screws, which were made by himself, being about 12.5 millimeters in diameter and 159 millimeters long. Thus the apparatus permitted determinations of the relative positions of stellar images, using either rectangular or polar coordinates.

In all his photographs of stars special precautions were taken to secure round images, and, so soon as the micrometer had been finished, Rutherford proceeded at once to train assistants in the use of the apparatus, and with their aid to begin the measurement of the plates in his own house.

Early in February, 1866, Doctor Gibbs had written to me of the completion of this micrometer and of the numerical results which Mr. Rutherford was deducing from the measures. On the fifth of that month I wrote to the latter, expressing my congratulations and assuring him of the high pleasure it would afford me to do what I could in computing his measured values and comparing them with determinations made by the ordinary methods. In reply he cordially invited me to undertake the work. Various plans for reducing the measured quantities to ordinary astronomical units and standards of reference were promptly arranged by correspondence; and at the beginning of March I visited him in New York and had the gratification of studying the various processes by which he had attained such brilliant success. It is needless to say how much I was impressed and instructed, and to what extent they kindled my enthusiasm. Mr. Rutherford placed in my hands a considerable number of measured angles and distances, and on returning to Cambridge I began the various trial computations without delay. The data were, if I am not mistaken, measurements of impressions of the Pleiades, taken February 21 and 26; but immediately after the methods of computation had been decided upon Mr. Rutherford sent additional series of measures of the same group from three plates of March 10, taken with exposures of four minutes, one minute, and twenty seconds and containing respectively thirty-seven, twenty-two, and fifteen stars, as well as excellent trails. These plates he believed to be somewhat better than the previous ones,

and not only were the images better, but more especially had longer practice given increased skill to the young women to whom most of the measuring was intrusted. The computations were continued and their results studied during the spring and early summer, and were essentially finished in July.

At the Northampton session of the Academy in 1866 Mr. Rutherford gave, on August 11, a detailed oral description of the methods and processes, telescopic and photographic, by which his impressions of stellar clusters had been obtained and of the micrometer with which he had measured them. On the same day it was my privilege to present a memoir containing the resultant determinations of the positions of the stars of the Pleiades, relative to *Aleyone*, which had been taken as center of reference.

Some allusion to these memoirs and to the history of their delayed publication has already been made, as also mention of the fact that the results for ten of the brightest stars of the group, besides *Aleyone*, were sent three weeks later to the *Astronomische Nachrichten* and there published in December, although without the details of computation.

The work thus begun was actively prosecuted by Mr. Rutherford, who continued to secure celestial photographs and measurements of the impressions; but his improvements in the methods were also unremitting. Within the next two years he had devised and constructed the photographic corrector. He replaced his 11 $\frac{1}{4}$ -inch achromatic by an object glass of 13 inches aperture, and adapted to this an exterior lens which could be readily applied so that the combination should serve for the chemically active rays. This obviated all necessity for removing the achromatic objective when the instrument was to be used for photography; while the facility with which it was attached permitted the telescope and mounting to be employed for visual or for photographic purposes at will. Only a change of the counterpoises was requisite when the corrector was put on and the camera substituted for the draw-tube. This new instrument he completed in 1868, the whole work having been done in his own house; and soon afterward important modifications were introduced in the construction of the micrometer.* The measuring screw was transferred to the microscope, and a fixed glass scale was added, having divisions corresponding to ten revolutions,

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so that the mechanical wear of the long micrometer screw became indifferent. This was first employed in March, 1871.

Thus the work of photographing and measuring was prosecuted until the year 1877, when failing health compelled him to desist. His physical constitution had for some time been delicate, and his scientific work, both intellectual and mechanical, was carried on in the face of serious obstacles. Moreover, frequent journeys on both sides of the Atlantic had been found necessary. Yet when he made to the observatory of Columbia College the princely gift of his apparatus, telescope, and collection of photograph plates, there were, among these last, 638 of stellar clusters, double and multiple stars, of which 190 had been elaborately measured; also a yet larger number of the sun, moon, and planets, and 174 of the solar spectrum.

It has been stated that the new method of observation was by no means enthusiastically received by astronomers. Most of them paid it no attention; others feared distortion of the relative positions of the stars as photographed, while others still distrusted the adequacy of the corrections to be applied; but the most serious criticism was based upon the supposed liability of the film to contract or expand, thus introducing new sources of error. Mr. Rutherford had carefully guarded against this danger by thoroughly albuminizing all his plates before applying the sensitive collodion, and had convinced himself by numerous and varied experiments that this danger was assuredly not a serious one; but, in the spring of 1872, a German astronomer, discussing the proposition to employ photography for observation of the transit of Venus—for noting which phenomenon preparations were then making in all astronomical countries—gave expression to strong distrust of the accuracy attainable by measurement of photographic plates. He gave, as results* of his own experiments, shrinkages of the films to a great extent by varying and irregular amounts and in different directions.

In an elaborate and very courteous article, published in the *American Journal of Science* on the 1st of December of the same year, Mr. Rutherford thoroughly disposed of these strictures. In addition to pointing out the probable explanation of the critic's results, he gave the results of his own experiments and measures with albuminized plates. He compared measures made when the

*A. N., No. 1884, lxxix, 188.

plates were taken from the camera, while they were still wet, with others made after they had become dry. He used negative impressions of dark lines on a white ground and of bright lines on a dark ground, some from which most of the collodion outside the lines had been removed while yet wet to facilitate shrinkage inward, and some from which most of that between the lines had been similarly removed to facilitate shrinkage outward, care being taken to insure that the same parts of the screw should be used in the measurements to be compared. The results fully justified Rutherford's confidence and corroborated his previous experience. He recognized a probable slight contraction of the glass plates, due to a cooling by evaporation as they dried; but he also showed how completely this could be allowed for by means of hygrometric observations.

In this connection I may be permitted to add a word as to my own personal experience. Having devoted much time during very many years to the study of a large number of stellar photographs and to the numerical values deduced from them, I cannot recall a single instance in which there has been reason to believe in any distortion of the film upon carefully albuminized plates. The employment of the much easier and more effective dry-plate processes of today makes the albuminization of the glass beneath the sensitive film more inconvenient than when the earlier wet plates were used, and as to the certainty of an unchanging record upon the bromogelatine film I feel less warranted in expressing a definite opinion. Two-thirds of my stellar photographs have been upon albuminized plates and by the wet process, and I have never found ground for doubt of the correctness of Mr. Rutherford's opinion concerning these.

Rutherford's latest scientific paper seems to have been a short one, dated June, 1876, and entitled, "A Glass Circle for the Measurement of Angles." After alluding to a communication which he had made to the Academy in April, 1870, describing the changes which he intended to make in the micrometer, and which have just been mentioned, and after reporting the great satisfaction which these modifications had afforded him, he stated that the success of the divided glass scale had confirmed him in a resolve of long standing, to try the experiment of substituting a glass circle for a metallic one in measuring angles with precision. He had owned a small spectrometer by Brunner furnished with a circle divided upon silver

and permitting readings to ten seconds by means of verniers. Of this he says :

"The diameter of the circle is small, not quite seven inches, and the inability to read smaller angles [than ten seconds] has always been its weak point. I have substituted for this metallic circle one of glass about ten inches in diameter, divided by Mr. Stackpole to ten minutes of arc and read by two micrometer-microscopes, magnifying seventy-five times; each revolution of the screw being equivalent to one minute, the drums being divided into sixty parts, read to seconds with easy estimate of fractions. Each degree line is numbered so as to be visible in the field of the microscope. I was able to furnish to Mr. Stackpole a well-tried diamond, which has made lines of the greatest delicacy, being much finer, as seen in the microscopes, than the spider lines by means of which the bisections were made. The advantages of this system are obvious, viz., perfection of surface, permitting a line of any desired fineness; facility of illumination, permitting an extension of the power of the reading microscopes to several hundred times; smallness of dimensions and consequent cheapness and avoidance of almost all the questions of flexure and local effects of temperature.

"I am convinced from the ease with which one second is read on my instrument, with microscopes only $4\frac{1}{4}$ inches long, including objectives and eye-pieces, that upon a circle of fifteen inches, provided with powerful microscopes, greater precision could be attained in the reading of angles than with the largest metallic circles now in use."

He appends four series of measurements of lines upon this circle, two made by himself and two by a lady assistant. They show the mean error of a single reading to have been so large as $0''.332$ for only one of the series of ten observations, those for the other three series having been respectively $0''.154$, $0''.270$, and $0''.286$.

This important and striking suggestion does not seem to have ever been followed up by other observers, but I entertain small doubt that the time for this will yet come.

During the later years of Mr. Rutherford's life the maladies which had undermined his strength and precluded the activity requisite for experiments or continuous effort became more oppressive. Soon after passing the age of three score years he found himself compelled to discontinue his scientific activity and devote himself to the care of his health and that of his family. It became imperative that his winters should be passed in warmer climates than that of New York, so that his residence during much of the year was in Florida, and he made protracted visits to southern Europe.

Yet he did not entirely abandon his celestial photography. In the list of plates given by him to Columbia College, as published by Professor Rees, there are twenty-three photographs of star-groups and six of the moon, taken in the early months of 1877, and twenty-five of Mars and neighboring stars, taken on five nights in August and September of that year. The latest of all were twelve impressions of the transit of Mercury, 1878 May 6. The bromogelatine process did not then exist.

Soon afterwards his observatory was dismantled, and the places where his study, workshop, and laboratory had been, and whence so many contributions to science and refined mechanical art had been given to the world, were surrendered to the demands of the huge and increasing city. As early as 1866 he had told me that the site of his uncle's orchard and grounds, where as a boy he used to play and roam at will, was already occupied by a population of at least 200,000 inhabitants. Even at the date when he mentioned this, his residence remained there, although the field of view for his telescope had become sadly limited. The tremors of the great city interfered with delicate experiments, and the atmosphere was seldom pure and transparent. Before the year 1880 the place had ceased to be a home of science and the abode of happy domestic life.

After this date a large portion of the limited time in which it was not imperative for him to reside in milder climates was passed by him at the rural estate, in northwestern New Jersey, to which his ancestors had given the appropriate name of Tranquillity, and to which he was strongly attached; but his physical strength waned steadily, and he became resigned to the sad consciousness that his scientific life was ended. In 1884 he presented to the observatory of Columbia College the 13½-inch telescope, with its accompanying corrector, which he had employed since 1868, and the improved micrometer used since the beginning of 1872. A few years later he completed the gift by adding the full collection of his photographic plates and the records of the measures made, all in perfect order and with the data needful for their proper computation. The total number of plates was 1456, of which 638 were stellar impressions, as has been already mentioned. A detailed catalogue of these was published in March, 1891, by Professor Rees, director of the observatory, who has stated that the measured values accompanying them fill some twenty folio volumes of two hundred pages each.

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With the plates and apparatus Mr. Rutherford furthermore made a liberal provision for continuing the work of measurement.

It would be needless to say that a brave and gentle spirit, like his, supported with patient calmness the manifest progress of disease and constant decay of strength, accompanied too by the domestic afflictions which few who reach old age can hope to escape. Of all these he had his full share, but they did not impair the serenity of his disposition or his never-failing kindness. The end of his sufferings came on the 30th of May, 1892, at Tranquillity, and just in the middle of his seventy-sixth year.

Such a man could not but exert a strong influence in the community, apart from that due to lineage and social position; and such influence was assuredly his, notwithstanding that he avoided public station and whatever savored of display. In 1863 he was appointed by Congress to be one of the fifty original members of this Academy. For 26 years he held the place of trustee of Columbia College, which institution conferred on him the honorary degree of Doctor of Laws. As a member of this board he was eminent for laborious and self-forgetful activity, and I am assured by competent authority that he was second to none in influence upon the guidance of the institution.

I will not attempt any minute analysis of the mental characteristics of our late colleague; for the simple narrative of what he accomplished makes such endeavor superfluous. An exceptional inventive power and boldness of device were combined in him with rare mechanical skill and the accuracy of an accomplished physicist, none the less well trained that the training was his own. His name will stand recorded as that of not only the pioneer, but the leader in more than one branch of what has become the department of astrophysics. He must be recognized as the father of celestial photography, and in spite of the cold indifference with which his first achievements were regarded by most astronomers, he kept steadily to his mission with an unostentatious disregard of obstacles until he saw it crowned with full success. Meanwhile his modesty withheld him from claiming the honors which the historian of astronomy must assign to him, and not until after his scientific career was accomplished did a new generation, for whose labors he had furnished much of the means, carry on the researches. Many for whom greatly increased facilities have been afforded by later progress, but whose acquaintance with the history of their science during

the third quarter of our century is not commensurate with their attainments, seem to have forgotten that they were not themselves the pioneers but only followers. It will be remembered that Rutherford's last photograph of a star-cluster was made in the early part of 1877; that his proposition of a photographic map of the heavens was in May, 1865, and coupled with a prediction of great advances in photographic chemistry. It was not until 1879 and 1880 that the processes were introduced which are employed at present and which so inordinately facilitate the practice of celestial photography.

Of our colleague's moral qualities I hesitate to speak as I am impelled to do, lest the words should have an aspect of exaggeration. Soundness and soberness of judgment in the various relations of daily life, no less than in scientific matters, freedom from the bias of prejudice, conscientious discretion, generous recognition of merit in others, and a rare unselfishness, were prominent traits in his character. While doubtless appreciating the importance of the results which he had attained by long struggles against unforeseen impediments and through persistent toil, he never seemed either to overvalue them or to see them in their wrong relations. He never lost sight of the fact that much of his work was dedicated to securing means for research, and that means are but the steps toward ends. The micrometers, by which accurate numerical data should be deduced from the impressions on the plates, were quite as earnestly the subject of his study and experiment as was the attainment of trustworthy impressions themselves; nor were the device and construction of the micrometers allowed to distract his attention from their purpose of obtaining accurate data for astronomical research. Even after he had been compelled to desist from other scientific work he caused the measurements to be assiduously prosecuted under his own supervision, and to the last day of his life he provided for the continuance of these measures.

His nature was tender, sympathetic, and generous in the extreme. To assist others who labored in the fields he loved, always seemed to afford him especial delight. In his few scientific controversies he never forgot the courtesy of a gentleman, nor failed either freely to concede what he could not maintain with full conviction or to maintain his ground with courageous frankness. Although modesty and dignified caution always prevented him from hastening the public announcement of his results before they had been carefully

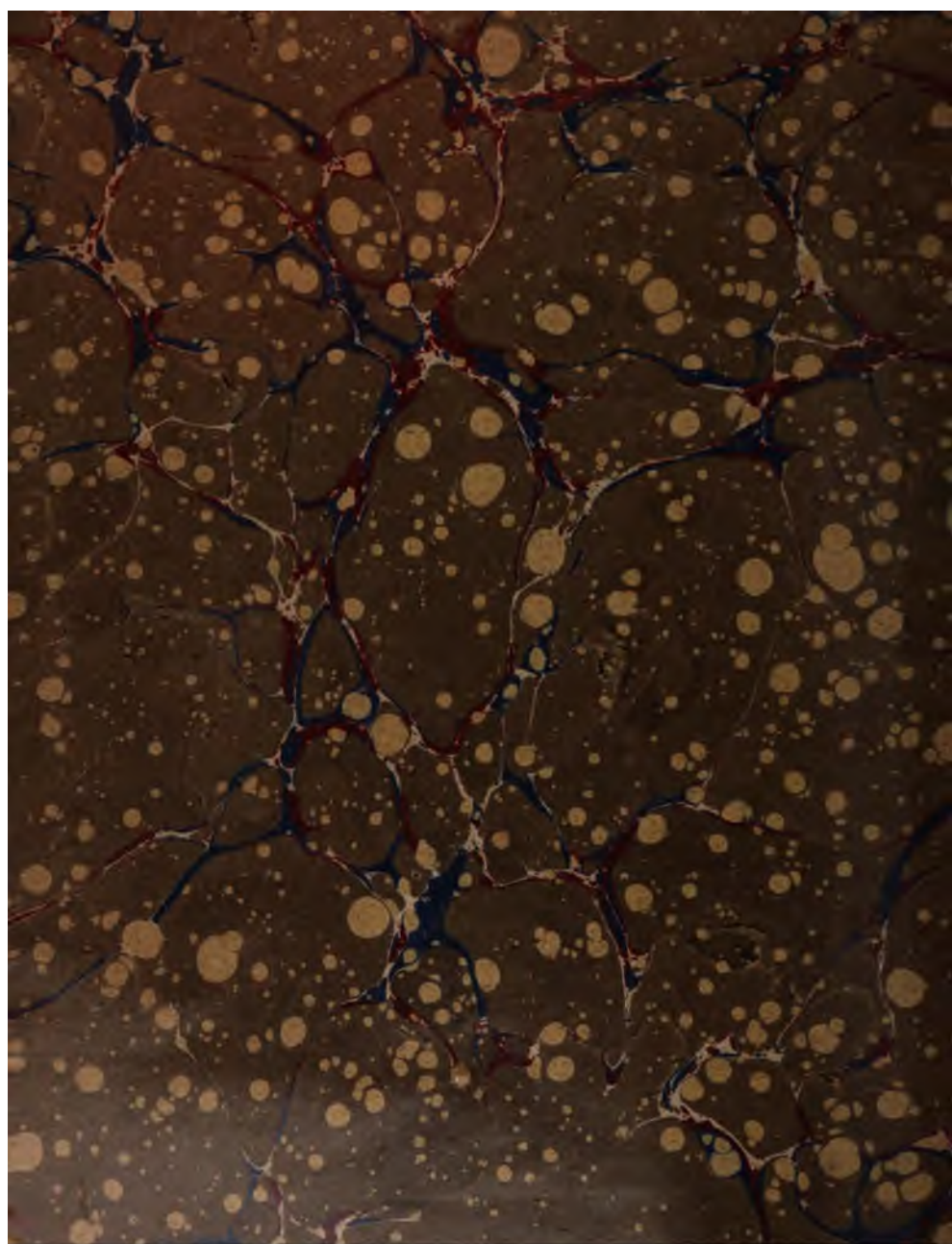
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tested and were evidently mature for publication, there was no jealous secretiveness in his nature, nor any reluctance to mention with perfect freedom the conclusions to which his researches seemed to be leading.

Such a man not only conferred honor upon our country and upon this Academy, but has left them a heritage—better even than honor—in the wide paths of usefulness he opened, in the incentive of his example, and in the memory of the dignified, unpretending modesty which manifested the unselfishness and sincerity of his scientific aims. His name will live in the annals of his science ; but this will be solely by reason of his deserts and not through the heralds of the market-place.

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